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## ESSAYS, MONOGRAPHS, AND CASES.

*Cholera Infantum—Diarrhœa and Entero-Colitis—their relations to each other—their Pathology and their Treatment.* By EDWARD H. PARKER, M.D. Physician to the Children's Department of the Demilt Dispensary, Fellow of the New York Academy of Medicine, &c. Read before the New York State Medical Society, February 4, 1857.

Can any apology be necessary for calling the attention of the members of a Medical Society to the subject of Cholera Infantum? If so, it must be found in the fact that the last census of the United States shows that 3960 deaths out of 323,023 (that is more than one per cent.) are caused by it; and that out of 106 enumerated causes of death, this stands in rank, as to the number of its victims, the *sixteenth*. I know that a great deal has been written about it; that almost all our journals contain, every year, one or more article upon it; while the learned Society (the New York Academy of Medicine) which I have the honor here to represent in part, has lately crowned with a prize of one hundred dollars an essay upon it.

Still, I beg your patient attention to the little I have to say; persuaded, as I am, of its importance. If I do not follow the beaten track—if I venture to make some suggestions as to errors in the nosology, the pathology, and the treatment of this disease, as commonly received—it is not from any desire to be captious, but from firm conviction of the truth of my position, slowly attained, gradually arrived at, never sought by me, but into which I have been forced.

I shall not so far presume upon the indulgence of the Society as to undertake here to recapitulate the history or to refer to the literature of Cholera Infantum. Here, at least, it is unnecessary, familiar as all the members of this Society must be with them; but I at once state the following propositions, as those generally received by the profession, or at least as generally given by our systematic writers upon the subject :—

1st. Cholera Infantum is a disease peculiar to this country, though most entirely unknown in Europe.

2d. That Cholera Infantum is almost entirely confined to large cities, and is rarely seen in the country.

3d. That the symptoms, course, and pathology of the disease, entitle it to a separate place in our nosological tables.

4th. That the treatment is to be distinct from that of diarrhœa on the one hand, and dysentery on the other.

5th. That "perhaps the most alarming symptoms are those of hydrocephalus, occurring in the advanced stages."

What I have to say can be most conveniently arranged by discussing these propositions in the order in which I have enumerated them.

The first inquiry is as to the truth of the proposition, that Cholera Infantum is almost unknown in Europe, though frequent in this country. But before discussing this point let me first quote descriptions of the disease, so that we may have a certain fixed point of comparison as to what constitutes it. I use the language of others, rather than my own, for a specific purpose. One writer* says :—

"When the attack comes on suddenly, it often commences with vomiting, and though in many instances the sickness does not recur frequently, yet sometimes the irritability of the stomach continues, for twenty-four or forty-eight hours, to be so extreme, that every drop of fluid taken is immediately rejected; and that frequent efforts at vomiting are made even when the stomach is empty. Violent relax-

* Treatise on Diseases of Children. pp. 388, 389.

ation of the bowels occurs almost simultaneously with the vomiting, and the child sometimes has as many as twenty or thirty evacuations, or even more, in the course of twenty-four hours. The motions are at first fæcal; but they soon lose their natural character, and become intermixed with slime, often streaked with blood. At first they are abundant, and are often expelled with violence; but before long they become scanty, though sometimes they still gush out without much effort on the part of the child. The character of the evacuations again changes: in the severest cases they not only lose their fæcal appearance, but become like dirty-green water, with which neither blood nor intestinal mucus is intermingled. Usually, however, when the first violence of the purging has a little abated, although some serous stools may still be voided, yet the evacuations consist chiefly of intestinal mucus, intermixed with a little fæces, and more or less streaked with blood. These scanty mucus stools are generally expelled with much straining and difficulty; a few drops of blood sometimes follow them; and once or twice, at an early period of the attack, I have known an infant void as much as a table-spoonful of pure blood."

"The skin becomes dry and very hot, though unequally so; the pulse is quickened, often very much so; the head is heavy; the child fretful and irritable if disturbed, though otherwise it lies drowsily in its nurse's lap, with its eyes half open, and scarcely closing the lids even when they are touched with the finger. Now and then, too, the disturbance of the nervous system at the commencement of one of these attacks of diarrhœa is so considerable, that a state of excitement alternates with one of stupor, that convulsions seem impending, and that there are distinct carpo-pedal contractions, or startings of the tendons of the wrist or fore-arm. The abdomen is usually full and tympanitic, but seldom very tender; nor does the child seem to suffer much pain, though sometimes a degree of tormina appears to precede each action of the bowels."

Another writer says:—"After a day of slight disturbance, with fever and vomitings, the diarrhœa appears and becomes very abundant; the face suddenly becomes changed, and the eyes excavated in the space of one night, like what it is said takes place in softening of the mucous membrane of the stomach, and the child rapidly succumbs. Recovery seldom takes place. In this case the symptoms gradually disappear or become prolonged. . . . .

"In most of the cases the commencement is less abrupt. The disease becomes developed, like catarrhal and spasmodic diarrhœa, with-

out attracting much attention. The child is indisposed and has slight relaxation of the bowels. The symptoms persist, and then the cause is discovered. They increase and become aggravated if their progress cannot be arrested. The patient daily becomes weaker, and gently arrives at that degree of weakness and emaciation precursory of death.

"It is only in exceptional cases that this disease rapidly runs through its stages. It usually lasts fifteen days: the mean term necessary to the cure. It sometimes lasts two or three months. Few children die before the completion of the tenth day. The greater number succumb by the end of a month. . . . Amongst those who are cured the disease scarcely ever extends beyond the third week."

Now I respectfully submit it to the members of this Society, if these are not good descriptions, accurate as can be drawn, of the disease which we know as Cholera Infantum. And yet both are from European writers, drawing from their own experience, and not quoting the language of others. One is West, of London, and the other Bouchut,* of Paris; and I have made these extracts to show that the first proposition, though so generally received, is an error. True, neither writer uses the words Cholera Infantum, but calls it, the one (West) *inflammatory diarrhœa*, the other (Bouchut) *entero-colitis*, and to those cases which are most marked he gives the name of *choleriform entero-colitis*. Both of these writers speak of the rapid termination of the disease, that is within two or three days from its commencement, as being rare in their countries; but so it is here, if we count the commencement of the disease to be the occurrence of diarrhœa, as I believe it to be, and shall attempt to show hereafter.

For the present I pass to the consideration of the second proposition, namely, "that this disease is chiefly prevalent in large cities, rarely occurring in the country."

I refer again to the mortality statistics of our last census upon this point. Here let me say that I am aware of the many sources of error to which we are exposed in basing any proposition upon these returns. Still the errors in the main counteract each other. In regard to this disease there is perhaps less occasion for fallacy, from the fact that the name is usually given by the medical attendant, "Summer complaint" being the more common term in popular use for diarrhœa. From these returns it appears that every State in the Union sends in its quota of cases of Cholera Infantum to swell the

* Bird's Translation. p. 469.

total to 3,960.* But one section of one State (the southern of Texas) is entirely exempt ; while of the territories Minnesota reports two cases of Cholera Infantum in a total of 27 deaths, and Oregon *one* of a total of 47.

But again : there are a few States in which the districts were so divided as that one of them should be made up chiefly or entirely of a city ; and we are thus enabled to compare the proportion of this disease as reported in the city to that in the rest of the State. These States are New York, Pennsylvania, Massachusetts, and Louisiana. From these returns it appears that in this State the total of deaths from Cholera Infantum is 447, of which only 257 were in the city of New York. In Pennsylvania there were 260 deaths of which 137 were in Philadelphia. In Massachusetts there were 331 deaths of which only 50 were in Suffolk county, which is almost entirely made up of Boston. In Louisiana 27 deaths from this cause give only 7 to New Orleans. Thus in four States, representing New England, the Southern and the Middle States, including the larger cities, we have for totals 451 deaths from Cholera Infantum in the large cities to 614 deaths from the same cause in the remainder of the States. That is about three-fifths of the deaths from this cause were out of the largest cities. So far then as the census goes we are justified in saying that the disease numbers quite as many victims in the country as in the city, or, at any rate, that a disease known to country practitioners as Cholera Infantum is as fatal in the country as the city. I appeal to gentlemen who practice in the country, especially in regions which are level, with a sandy soil and shut in by hills, whether or not the symptoms described by our writers as being those of Cholera Infantum are not constantly seen by them during the hot weather. I know that the best marked cases which have ever fallen under my observation are those seen by me in New Hampshire and Massachusetts, and no symptom described by city writers was absent

* *The number of deaths given for each State are as follows:*

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| Alabama, | 42 | Louisiana, | 27 | Pennsylvania, | 260 |
| Arkansas, | 14 | Maine, | 52 | South Carolina, | 64 |
| California, | 1 | Maryland, | 166 | Tennessee, | 17 |
| District of Columbia, | 25 | Massachusetts, | 331 | Texas, | 12 |
| Connecticut, | 85 | Michigan, | 13 | Vermont, | 6 |
| Delaware, | 8 | Mississippi, | 114 | Virginia, | 150 |
| Florida, | 10 | Missouri, | 83 | Wisconsin | 14 |
| Georgia, | 75 | New Hampshire, | 47 | Minnesota, | 2 |
| Illinois, | 141 | New Jersey, | 105 | New Mexico, | -- |
| Indiana, | 156 | New York, | 447 | Oregon, | |
| Iowa, | 28 | North Carolina, | 74 | Utah, | -- |
| Kentucky, | 163 | Ohio, | 232 | | |

from them. I contend then, that the second proposition, though generally received, is incorrect.

"The third proposition is that the symptoms, course, and pathology of the disease, are peculiar; and entitle it to a separate place in nosological tables."

The characteristic features of Cholera Infantum are thus defined by Dr. J. Forsyth Meigs, whose remarks on this disease are, on the whole, among the best that have been published in this country. "These are its occurrence in very young children, and in the Summer months; the evidences in the early stage of violent irritation and hyper-secretion of the gastro-intestinal mucous surface, and at a later period of inflammation, ulceration, softening and thickening of the same surface, particularly of the ilium and large intestine; its chief symptoms are vomiting and purging; fever, generally of a remittent type, varying often with collapse; rapid emaciation; and towards the close, violent cerebral symptoms." (Meigs, p. 28.) Authors describe affections of the bowels under three names, viz: simple diarrhœa, Cholera Infantum, and entero-colitis, and each one of these may be accompanied by these chief symptoms, to wit: vomiting and purging, remitting fever, collapse, emaciation, and "violent cerebral symptoms." How then can it be known when diarrhœa ceases and Cholera Infantum begins; or when does entero-colitis become Cholera Infantum. The fact is, that the continued and simple diarrhœa, when aggravated without any change in its nature, becomes violent, so that it is called Cholera Infantum. Entero-colitis commencing suddenly with vomiting and purging, is Cholera Infantum in common professional parlance, but no more a separate disease than is the first stage of pneumonia a separate disease from the last. It is rarely the case, if ever, that a simple diarrhœa runs on to be severe without the occurrence of vomiting, feverish exacerbations, collapse, and the other enumerated symptoms, and it is therefore necessary to say that if Cholera Infantum is a separate and distinct disease, it follows diarrhœa very frequently. On the other hand, entero-colitis is usually attended by vomiting as well as purging, and is thus said to be preceded by Cholera Infantum. So it is with the pathology of the disease. It is entirely impossible for any one to say, simply by an inspection of the post mortem appearances, whether or not the patient had died of Cholera Infantum or simple diarrhœa on the one hand, or on the other to distinguish between Cholera Infantum and entero-colitis. One school of writers have dwelt much on the altered condition of the liver, which is said to be enormously enlarged in some cases, and with all the intermedi-

ate shades of departure from health. But this scapegoat for so many evils is not justly blamed. If Cholera Infantum is a peculiar and distinct disease, it is eminently an acute one, for it is improper to assign to it all that train of symptoms which drag after it for months. In this time the liver cannot enlarge, for it is not a sponge which can be filled with blood and be made to swell up and puff itself out to occupy half of the abdomen in three days or in three weeks—as seems to be intimated by several writers. This error must have arisen from neglecting to take into account the fact that in infancy the liver is proportionably much larger than in more mature life. As I have said of the symptoms, so it may be said of the pathology—it is impossible to draw a line on the one hand between Cholera Infantum and simple diarrhœa, and on the other between Cholera Infantum and entero-colitis. The disease has no distinct pathology, as will be evident if we compare the descriptions given of the pathology of this disease by our writers with those given by European writers of the pathology of diarrhœa and entero-colitis. I claim then, that the third proposition usually received, is incorrect, and that there are no separate symptoms, course, and pathology of this disease, but that Cholera Infantum is in fact a name given to a particular condition arising in other diseases.

Of course, if my position is correct, the fourth proposition, viz: that the treatment of the disease requires to be peculiar, goes with the third, for if there are no peculiar symptoms, course, and pathology, there is no separate and peculiar treatment. The proposition was stated, however, to express the common belief upon the subject, and I suppose I am correct in saying that nine-tenths of the profession would say so. As large a proportion would insist upon the necessity of using calomel or some other preparation of mercury, and that with more or less freedom. Some use it often and in large doses, some in full doses and but once, others use smaller doses, while still others approach the infinitesimals in appearance though not in reality. The doses vary from five grains to the sixteenth of a grain, but calomel is the desideratum with them, and calomel must be used. To cite my authorities for this, would be to cite almost every writer upon the subject; few, if indeed any writers in our own country being content to advise entire abstinence from the use of this drug. Many of them insist as strongly upon the disuse of astringents, and on this point too, I am at issue with them. I have no particular fondness for astringents, neither have I any dislike of calomel, much less a prejudice against so excellent a remedy. But it certainly is a

very potent one even in small doses, especially when given frequently, and I do not hesitate to say that it has often done in this disease great and grave injury.

The 5th proposition, viz: that "perhaps the most alarming symptoms are those of hydrocephalus occurring in the advanced stages," is in the very words used by Dr. George B. Wood,* in his excellent treatise on the Theory and Practice of Medicine, and is, I suppose, expressive of the common opinion. I confess that I should not have ventured to make such an assertion a year ago, for the simple reason that after the clear descriptions of the actual cause of the cerebral symptoms which Marshall Hall gave more than fifteen years ago, and which have been since repeated by Gooch, and more lately by West, it would seem improbable that the condition of anæmia or exhaustion should not be recognized as the cause. That it is not so, the prize essay to which I have before alluded, bears ample testimony, while the occasional favorable notices of that publication in which even eagle-eyed reviewers do not detect this error, give farther confirmation of it. *Hydrocephaloid* is the actual condition, not *hydrocephalic*, and that antiphlogistics (as leeching, blistering, &c.) have not been successful in the essayist's hands, is no wonder. The cool head; the depressed fontanelle, the previous or continuing exhaustive disease; the rapid improvement under the use of tonics and stimulants, all of these should have opened the eyes of practitioners to the actual cause of the symptoms. This is neither the place in which, nor the audience to which I ought to repeat what has been so well said by others; but I may simply urge any who have not become fully convinced of the correctness of the distinction, to read again what West has said so well, and to study carefully the next cases of the disease which come under their charge.

It would not be proper for me, having said so much that is negative, to refrain from a positive statement of my own opinions concerning the topics on which I have touched.

Diarrhœa in children (that is, too frequent and too fluid evacuations from the bowels) may be of all grades, from the slightest to the most grave. Especially apt to affect teething children, particularly those that are weaned, and to appear during the hot season, its symptoms (in general the same) may at any moment be aggravated and exaggerated by the continuance of the very causes that produced it. Nausea is a constant accompaniment of the diarrhœa, and the aggravation of the diarrhœa, whether gradual or sudden, is attended by a simi-

* Practice of Medicine, vol. 1, p. 700.

lar increase of the nausea, vomiting soon succeeding. If then a rapid or sudden increase of the diarrhœa occurs, which is always attended by an increase of the watery portions of the dejections, it is almost inevitable that vomiting should occur. Excessive thirst, intolerance of the blandest food, rapid prostration and emaciation are the accompaniments and results of this condition. Unfavorable hygienic conditions serve to increase the liability of children to the disease, while they at the same time retard, if they do not entirely prevent recovery from it. Pathological Anatomy shows to us throughout a continuation of the same condition, increasing in severity and gravity with the corresponding increase in the symptoms.

When death occurs from the exhaustion produced by the profuse vomiting and diarrhœa, a condition to which is given in this country the name of Cholera Infantum, we find the intestines to contain more or less of a soft, usually light, yellow fæcal matter, and the stomach a fluid resembling a thin gruel. The walls of the stomach are natural, unless the epithelial lining be a little too easily removed,—the epithelial lining of the small intestine, and sometimes of the large, being in a similar state. The walls of the intestines are almost translucent, bloodless, and apparently thin. Throughout their whole extent the solitary and agglomerated glands are very prominent, setting up almost like beads from the surface. The mesenteric glands are not changed. The liver is pale and anæmic, the gall bladder containing more or less of thin bile. General anæmia of the organs is the only other observable change from health. The brain itself is in the same condition, a passive congestion of some of the larger veins of its base being the utmost change notable in the blood-vessels. Where the prostration has continued a long time, we have a little serous fluid in the cavity of the large ventricles. Such is a general view of the pathological anatomy of these cases. The pathologist must therefore be content with saying that the death is from exhaustion, and not from anatomical lesions of the organs. From the prominence of the solitary and agglomerated glands, it has been supposed that they are the seat of the disease, but a careful inspection will show that they are only filled with their peculiar secretion. Being without outlet, as Kolliker and others have distinctly shown, their excessive secretion only distends them, while the exsanguine condition of the neighboring tissues thins *them*, and thus gives to every independent prominence like the follicles, still greater exaggeration. Hyper-secretion is the only thing of which we have any evidence in the bowels, and hyper-secretion will account for the symptoms and the

death. Back of this we must confess we cannot go, and irritation, inflammation, nervous force, or anything else may be invoked to explain it. Evidence of either we do not have.

If instead of this sudden termination, the case runs along for weeks or months, we then have in addition to the other appearances abrasions, and even ulcerations in some rare cases, of the mucous surface. Punctate injection more or less abundant, and more or less general, though usually quite limited, is visible. The solitary and agglomerated glands have either disappeared or are in the same condition of distention as that before pointed out. If it be the former, there results one of those depressions which are described as ulcerations, but which are without the characters of an ulcer. Occasionally there may be seen patches of an apparently uniform redness, but closer examination shows that it still is of the punctate form. This is the condition to which, following the European writers, I give the name of entero-colitis. But this follows directly upon the continuance of aggravated diarrhœa. Other diseases may of course complicate it, but I speak now of pure and simple cases, of which many may be noted by every one engaged in the treatment of young children.

The localities in which the disease most frequently occurs, as well as the class of patients who are especially liable to it, point out the influences against which we are to guard. Although the disease is marked, in many cases, by febrile exacerbations early arising, there is no reason to suppose it to be allied to periodical diseases of a malarious character. I have seen it as evident in regions where no intermittent fevers are found, as in those regions which abound in them. It is noteworthy that these exacerbations are not of a high grade of fever; and one who looks for a frequent and strong pulse, with flushed features, and the other usual marks of fever, will not find them. Instead of this, there is a very quick and frequent pulse and some increase of the heat. It is usually quite transient in such cases. In those of a more chronic character it is often more protracted and rises higher; that is, this is more marked in entero-colitis than it is in diarrhœa. The impure air of cities is justly, as well as generally, blamed for much of the disease ; but it is not the impure air which has the largest influence. The classes of patients who are particularly exposed to it in large cities, are those who are also under the most unfavorable hygienic conditions. Poor and squalid, the mother must turn every moment to a good account in making a living; or else, drunken and vicious, she spends in dissipation the time which should be given to her infant. In either case, her impov-

erished and scanty milk compels her to feed her child, even if another pregnancy does not urge to the same course. Poor cow's milk, crude gruels, and panadas, or bits of "every thing that is going," are the materials poured into the intestines, and these the child can not properly digest. The house in which they live is crowded, and close, and damp ; ventilation is a thing unknown or uncared for, and bathing is a luxury that is rarely indulged. Dentition, too, complicates matters ; for in no class are the disturbances produced by it greater, and in no class is there more frequent or strenuous objection to lancing the gums. In addition to this, insufficient light reaches the child, and in its etiolated state it is still more unable to contend with other unfavorable influences. But I have shown that it is not confined to large cities. The proportion of poor persons increases more rapidly in large cities than in direct ratio to the increase of population, and therefore the disease prevails in larger proportion ordinarily. This is not uniformly so, however, and the census to which I have so repeatedly referred, shows this in one instance quite singularly. In Massachusetts the whole number of deaths from Cholera Infantum is reported as 331, of which only 50 were in Suffolk county, which is almost synonymous with Boston, the single town of Chelsea being included within its limits. Now Essex and Middlesex counties, which make another district, and have about the same population, give 113 deaths from Cholera Infantum—more than twice as many. Boston, it is well known, is so built on hills that it is thoroughly drained ; and, at first sight, it might be thought that this was the cause of the slight mortality. But Essex and Middlesex counties are not deficient in drainage, are not malarious, and are commonly considered as healthy districts as any. But these counties abound in localities in which the soil is light and porous, perhaps sandy, and where these lie upon the rivers, especially the Merrimac, they are shut in by hills at a little distance from the river. Here the heat during the middle of the day is intense, and often continue through the night. Under these conditions severe forms of diarrhœa develop themselves rapidly; that is, if it be preferred, Cholera Infantum prevails, and fatally. The same is true all up the Merrimac river wherever these conditions unite. Manchester, N. H., where, from the very sandy nature of the soil, and the rapid growth of the town, all these influences are in excess, is swept with this disease every Summer, to such an extent that a similar visitation in New York would be more terrible to children than the most malignant yellow fever. The same is true of Concord and Nashua,

though not to quite the same extent, for the conditions are somewhat less aggravated. This fact, and the want of similar localities in Vermont, seem to me to account for the notable difference in the proportion of deaths from this cause, given in the census—Vermont having six and New Hampshire forty-seven—for in other respects the situation of the two States and the hygienic conditions of their people are very similar. The exemption of Boston I believe to be largely due to the refreshing sea breezes which prevail during the day and reduce the air to a delightful temperature. Serious as an East wind in that city is to delicate persons in the month of March, in the month of July it is exceedingly refreshing.

But I hasten to close my remarks, already too protracted, I fear, by a discussion of the treatment which I believe to be best adapted to the disease.

In the first place, I would lay it down as a rule, that diarrhœa is not to be allowed to run on in teething children. It is to be controlled, not to the extent of producing constipation, but to that extent that it ceases to be strictly a diarrhœa. Division of the gums in a proper manner and always when called for; the use of chalk mixture, alternating occasionally with small doses of the aromatic syrup of rhubarb; early resort to tonics, carminatives, and stimulants, together with the wearing of flannels next the skin, especially so as to cover the abdomen completely, are the remedies. Careful diet (which should be the mother's milk if possible), regular exercise in the open air during the early part of the day, and protection from the excessive heat of noon, are the hygienic necessities. Daily warm baths and frictions, the baths to be once every week of salt water, are also important. And under these influences very many children are kept safe from the dangers of the more advanced disease.

If the diarrhœa is more aggravated, though without severe vomiting, there may be given chalk mixture, with some carminative, astringent and tonic. My own preference being for a combination of chalk mixture, tincture of catechu, compound tincture of cardamoms, and compound tincture of cinchona. To this mixture I sometimes add a little paregoric. At the same time the flannel which covers the abdomen may be sprinkled with brandy or tincture of camphor.

When vomiting occurs with profuse discharges, that is, when the stage arrives to which the name Cholera Infantum is given; if it comes suddenly, and there is a suspicion of undigested food in the bowels, a mild purgative may be given, and the best is syrup of rhubarb. This, however, should not be attempted more than once,

unless the stomach has become quiet. Many, it is well known, prefer calomel in a cathartic dose, and it is easily taken and does not nauseate by its taste, but it is, to my thinking, too violent and irritating a cathartic. The vomiting should be allayed by the use of bits of ice, (not teaspoonfuls of cold water, for that will not do it so rapidly,) which may be swallowed whole if desired; by mustard poultices, small ones, to the pit of the stomach; by quiet and not attempting to fill the stomach with food. The warm baths should be continued; mild opiate injections or suppositories may be used, and an early resort be had to stimulants when symptoms of exhaustion appear. These may be given in almost any form, certainly in any that agrees with the stomach. At one time they will be best borne cold, at another hot; sometimes diluted with water only, sometimes with milk, as punch or whey; but all of these forms are to be selected from according to the occasion. At times these will not stay upon the stomach in any form, and then it is that bathing with diluted spirits should be carefully attended to. In this disease it is important to continue to try to save life as long as a spark of it remains, for, from the most unpromising condition recovery sometimes rewards the diligent and faithful attendant.

When the disease becomes chronic it is necessary, to pursue a similar, and yet in some respects, a different treatment. The chief difference is in the mode of procedure when there occurs a series of bloody discharges. That is when the colitis is more severe and prominent, and when the condition approaches that of dysentery. For several years, I have in this state of affairs, used with much satisfaction, a mixture of about ten grains of blue mass rubbed up in two drachms of syrup of rhubarb, to which is added one-half teaspoonful of paregoric and four ounces of chalk mixture. Of this, a teaspoonful every two or three hours, is the dose. The blue mass certainly does not act like the calomel, not producing, in purgative doses, so great prostration, and in small doses it does not lessen the proportion of fibrin in the blood, as is the case with calomel. Hence calomel is the better agent when the intention is to arrest the exudation of plastic deposits, as in the inflammation of serous membranes. It is probable that the portion of mercury in the blue mass which becomes converted into the black oxide, is the beneficial agent, and in this idea I have been confirmed, by noticing the effects of the black oxide of mercury prepared by means of ammonia. This, which some homœopathists use under the name of "soluble mercury," is a valuable remedy, and is worthy of more general attention, from its good effect and from

the facility with which it is taken by children when rubbed up with sugar. It is of course understood that I do not recommend any dilution for the sake of potentization.

In this state opiates, if none are contained in this mixture, are often of very great benefit when given with discretion, that is, in proper dose not too frequently repeated. Notwithstanding the prejudice against their use, I hardly know what we could do without them; certainly they expedite recovery. Another prejudice exists against the use of chalk mixture, but I have yet to see or to learn of one who has seen the concretions ascribed to it after use in this disease. It should not be constantly continued for months, for the condition which calls for it does not require that. I do not dare to say how large a quantity of it I have prescribed during the last Summer only, but it has been very large, and I have yet to see its bad effects.

Exposure to the fresh air is still more important, if that be possible, than ever, and it should be regular and constant. I could speak of very striking instances of its benefits, but I fear I become wearisome. It does not absolutely require that the child should be taken into the country. Many of my patients are sent to the ferries to cross them so that the cool fresh sea-breeze may fan them, and it acts sometimes like magic to raise their drooping heads. Of bathing, too, in warm water, I could give illustrations, the most striking being where a gentleman, whose child was under another practitioner's care, ventured to direct one leg to be washed hastily, contrary to the physician's advice, although the child had been sick several weeks. He distinctly affirms, that the leg so washed, got so much the start of the other, which was not similarly treated for a few days, that it took the latter a long time to catch up with it in strength and in flesh.

In conclusion I present these propositions as expressing my views of this matter, in contrast to those which I first gave as expressive of the opinions more commonly entertained.

1. The condition to which the name of Cholera Infantum is given, is not a separate and peculiar disease, but a collection of symptoms attendant on certain stages of other diseases.

2. This condition is recognized by European authors in their treatises, though very properly it does not receive a separate title.

3. That the principles of its treatment are the same as those of diarrhœa and entero-colitis.

4. That Hydrocephalus is rarely if ever an attendant or sequent of it, but that the hydrocephaloid disease is very usual.

*Selections from Favorite Prescriptions of Living American Practitioners.* By HORACE GREEN, M.D., LL.D., &c.

*Astringents.*

Astringent medicines are usually defined, substances which coming in contact with living organisms corrugate and condense their fibres, and thus diminish the action of secretion and excretion. As astringents exert, ultimately, a tonic influence on the human body, and as some tonics often prove astringent, many writers have considered tonics and astringents as identical. Tannin, which is an important ingredient in many astringents, is not indispensable to tonics; in many respects, however, they appear to be very nearly allied. Some of the most powerful of the astringent remedies are derived from the mineral kingdom, but by far the largest and most important portion of this class of medicinal substances is obtained from the vegetable kingdom.

Caution should be observed in adopting the astringent plan of treatment, in most diseases. In acute inflammations, in all affections which are accompanied with any degree of fever, or even those of a chronic nature attended with serious alteration of the tissues, medicines of the astringent class, are likely to prove injurious.

Some of the more powerful of the astringents, are frequently employed locally for the purpose of arresting hæmorrhages; they are then called *Styptics.*

In some cases where astringents are indicated, if irritability exists, opium, although belonging to the division narcotics, may be administered with great advantage, either alone or combined with some more immediate astringent.

As the most convenient preparation of opium for the ready formation of pills, we recommend to practitioners the *officinal* mass of Dr. Tully:

℞. Opii pulveris - - - ℥i
Camphoræ pulv. - - - ʒi
Saponis Alb. - - - ʒvij
Subige in massam. divid. equal. in pil. ccxl.

Each of the above pills will contain one grain of opium.

This mass will remain of the same consistence for a great length of time, and may be readily formed into pills of any desired size; or other medicinal substances, or astringents, can be most conveniently combined with it.

℞. Pilula. Opii Officinalis - ℥ss
Plumbi Acetatis - - - ℥i
Ipecac pulv. - - - gr. xv
M. Fiat massa in pil. xxx divid. cap. unam pro re nâta.

These pills, administered once in two or four hours, according to circumstances, are useful in hæmoptysis, and other internal hæmorrages.

℞. Pil. Opii Officinalis - - ℥ss
Argent. Nitratis - - gr. viij
M. Fiat mass. in pil. xxx divid. quarum sumat unam terve in die.

In chronic diarrhœa, and in the latter stage of dysentery, the above pills have been successfully employed by us to allay the irritation of the intestinal membrane.

In active hæmorrhages, the subjoined astringent pills have been much extolled :—

℞. Plumbi Acetatis - - - ℥ss
Digitalis - - - - ℈i
Opii pulv. - - - - gr. v
Conservæ Rosæ - - - ℥ss
M. Fiat massa divide in pil. xx et exhibe unam ter quaterve in die.

An experienced physician of this City has employed for many years in pulmonary hæmorrhage the following astringent mixture :—

℞. Plumbi Acetatis - - - ℥j
Tinct. Opii - - - - f. ℥ij
Aceti - - - - f. ℥v
Aquæ Font - - - - ℨiij
Misce quarum capiat cochl. una parv. pro re nâta.

This mixture is declared to be more efficacious in arresting hæmorrhage from the lungs, than the lead and opium are, when administered in the form of a pill. The medicine may be exhibited in teaspoonful doses once in two hours, or oftener if required.

℞. Plumbi Acetatis - - - ℥j
Tinct. Opii - - - f. ℥iss
Aquæ Cinnamon.
Aquæ Fontanæ - - - aa ℨij
M. Fiat mistura, sumat cochl. parv. ter quaterve in die.

In both uterine and pulmonary hæmorrhage, we can recommend, from experience, the above mixture as a most excellent remedy ; and less disagreeable than the preceding mixture.

℞. Tinct. Catechu - - - ℨiss
Aquæ Cinnam. - - - ℨvss
Tinct. Opii - - - ℥ij
Syr. Simp. - - - ℥j
M. Fiat mistura, cujus capiat cochl. mag. pro dosi.

In the last stage of Dysentery, or in protracted diarrhœa, attended with profuse evacuations, the above mixture may be administered after each liquid evacuation, often with great benefit.

℞. Zinci Sulphat. - - - ℈ij
Opii Extracti - - - gr. x
Confectionis Aromat. - - ℥ss
Syrupi q. s. ut fiant pil. xl sumat unam ter die.

The above pills may be employed advantageously in some forms of chronic bronchitis, when the expectoration is profuse. They are also a valuable astringent and tonic remedy in the treatment of leucorrhœa. Under the same circumstances the following pills, if preferred, may be administered :—

℞. Cupri Sulphatis - - - gr. x
Opii Extracti - - - gr. x
Confectionis Rosæ - - ℥j
M. Fiat massa in pil. xxx div. sumat unam ter quaterve in die.

In hæmorrhage from the kidneys or bladder, or from the intestinal mucous membrane, tannic acid is by far the most effectual astringent of this class of remedial agents.

℞. Acidi Tannici - - - ℥i
Extracti Gentian. - - ℈ij
Misce in pil. xx divide, cap. unam tertiis vel quartis horis.

Combined with opium, tannic acid is often successfully administered in the treatment of uterine hæmorrhage, and in protracted diarrhœa.

℞. Acidi Tannici - - - ℥i
Extracti Opii - - - gr. x
Conservæ Rosæ - - - ℥ss
M. Fiat massa in pil. xxx divid. sumat unam ter quaterve in die.

In uterine hæmorrhage one of the above pills may be administered every hour.

℞. Decoc. Uvæ Ursi - - ℥iv
Acidi Tannici - - - ℥j
Tinct. Opii - - - - f. ℥ij
Syr. Acaciæ - - - ℥ij
Misce. Fiat mistura sumat cochl. parv. ter in die, vel sæpius.

In albuminuria, and in chronic catarrh of the bladder, the above astringent mixture is a most valuable remedy.

By some practitioners the gallic acid is considered preferable to tannic acid, inasmuch as the administration of the latter is apt to be

followed by constipation of the bowels. Gallic acid is said not to produce this effect. The following mixture may be employed:

℞. Acidi Gallici - - - ℥j
Mucil. Acaciæ.
Syrupi Aurantiæ - - - aa ℥ij
Aquæ Fontanæ - - - ℥iv
Misce. Fiat mistura, cujus sumat cochl. j mag. ter quaterve in die.

In menorrhagia, hematuria, and other internal hæmorrhages, the above mixture may be given with great advantage.

The following preparation, which does not differ materially from the above, is a favorite prescription of our colleague, Prof. Barker, in menorrhagia.

℞. Acidi Gallici.
Acaciæ pulv. - - - aa ℥ij
Syrupi Tolutan.
Aquæ font. - - - - aa ℥ij
Misce. Fiat mistura, sumat cochl. parv. pro dosi.

Although belonging more properly, perhaps, to the class narcotics, we shall under the head of astringents speak of a medicine, which, after its employment for many years, not only by ourselves, but by many professional friends, we can confidently recommend as one of the most useful, and, in one class of maladies, the most certainly remedial, of any of our therapeutical agents.

℞. Tinct. Camphoræ - - ℥iss
Tinct. Capsici - - - ℥ss
Spiriti Lavendul Co.
Tinct. Opii - - - aa ℥i
Misce. Fiat mistura capiat M. xx vel xl. pro re nâta.

Not only in the milder forms of diarrhœa have we found the above mixture an excellent remedy, but it has proved in our experience, most valuable and efficient when promptly administered on the occurrence of the "premonitory diarrhœa," in the early stage of Epidemic Cholera.

During the prevalence of the cholera in this City, in 1849, and again in 1850, we administered this mixture in a large number of cases of cholera, in the access of the disease, with entire success. On the occurrence of the earliest symptoms of the choleraic diarrhœa, it should be given in doses of from thirty to sixty drops, every hour (or oftener if required), confining the patient *strictly to a horizontal position*, until all tendency to the diarrhœa is entirely overcome. The adoption of other measures will, of course, be required, if the disease is much advanced.

When travelling in Summer, we are accustomed always to take a small phial of the above mixture with us; as this medicine is quite sure to arrest, in children or adults, the intestinal irritation which in the warm season is so liable to follow a change of diet and the drinking of a different, and perhaps a *harder* kind of water than that to which we had been accustomed.

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*Objections to Exclusive Systems in Medicine.* Extract from an Address delivered at the close of the Session of 1856–'57. By Timothy Childs, M.D., Professor of Anatomy in the New York Medical College.

In no period of the history of medicine has there been witnessed a *progress* at all comparable to that which has characterized the last quarter of a century. This was to be expected. Medicine can improve only as the collateral and tributary sciences improve—as these are perfected and applied to the study of our science, and as nature is more severely interrogated by better and more thorough methods of investigation. Medicine does improve, but it improves slowly. Eager minds are not content to labor and to wait, and tread slowly and painfully the toilsome path of induction. Perfect systems of medicine are started which boldly challenge your acceptance. . . . But of all these full-blown schemes of medicine, there are but *two*, that I know of, that possess any fascination for the cultivated and well instructed young physician. These are, the exclusive system of the partisans of the "numerical method," and the exclusive system of the partisans of Homœopathy.

The accurate and beautiful method of observation, known as the "numerical method," I do not stand up here to decry. It is its *abuse* by insisting on it as the *one* and *exclusive* method in the cultivation of medicine against which I protest.

I pass next to the second exclusive system, the doctrine and practice of Homœopathy. . . . Now I object to this exclusive system:

1st. That it ignores the very existence of the "*vis medicatrix naturæ*"; the tendency and the capacity, under favorable conditions, with which a kind Providence has endowed every organ, and the totality of the organs composing the human system to return to a healthy structure and function, when these have been impaired or deranged. Hahnemann frankly avows his total skepticism as to the

curative powers of nature (Organon, pp. 30–34). Is this so? Has this been the error of the regular profession, or rather has not their error been that, while acknowledging themselves the "ministri naturæ," they have, with Hahnemann, fancied themselves the "magistri naturæ." May I not go farther and ask, is disease ever cured in any other way? Should the physician ever do more than assist her whom the great poet so justly styles "excelling Nature."

"2d. Homœopathy ignores all *entities* in disease, and deals only with the subtile vital power. It ignores, for example, the condition of the blood in disease. Take the case of a worker in lead: he inhales the lead fumes—the poison is absorbed by his lungs, it circulates with his blood to his stomach, and he has horrible colics; it circulates to the muscles of his arms, and he has paralysis; he dies. Tanquerel finds the lead in the coats of his stomach and in the extensor muscles of his wrists. Isn't *lead* an *entity?* Taught by experience, we say this poison of lead is the cause of the disease. We say, "*tolle causam cessit affectus.*" We put our next patient in a sulphur bath; we show him the sulphuret of lead on the skin; we give him the iodide of potassa, and, pari passu, with the elimination of the poisonous mineral the patient regains his health. But the homœopathist overlooks all this; he asks not what is at fault with the machine, to apply the proper corrective, but what are the *symptoms*, and what drug is homœopathic to the symptoms. But this, you say, is a case of *poisoning*. But are not *gout* and *rheumatism* and *typhus* and *ague* equally cases of poisoned blood? Admit, frankly, that we do not yet know the history of some of these as well as that of lead poison. But twenty-five years ago we knew less of lead poison than we now know of the poison of rhuematism, or the poison of gout. Take another illustration: suppose a case of *chlorosis*, pure and simple. Andral and Gavaret have weighed for you the liquor sanguinis in scales, and measured the corpuscles in a balance. They find that the proportion of corpuscles is reduced, say one-third. We administer the appropriate pabulum for the manufacture of these red globules, say a grain of *iron* twice a day. The proportion of corpuscles steadily increases; the strength and health of the patient increase in the same ratio. We suspend it for a week, by way of experiment; the patient stands still. We resume it again; the proportion of corpuscles steadily increases, and you don't recognize, in your ruddy patient, the pale girl for whom you ordered the box of little pills of iron three months ago. We take a case of *scurvy*—where, from deprivation of the vegetable juices and an insufficient diet,

the blood is impoverished; we supply what is lacking to the vital fluid, and without other medication the patient grows well and strong. Dr. Kane's last *scraped potato*, and *raw walrus*, were certainly "entities," and certainly did not act on the universal law of cure of exclusive homœopathy. "*Similia similibus curantur.*" The illustrations might be indefinitely multiplied.

3d. And closely connected with the homœopathic dogma just adverted to, that "the causes of disease can not possibly be *material*," as Hahnemann phrases it, is another dogma, that the totality of the *symptoms* represent, constitute, are the disease. (Organ. p. 85.) This, indeed, is an essentiality of the exclusive homœopathic system. But is it so? In a very common disease of the *hip joint*, the pain is often referred to the *knee*, and very often the knee is even *swollen;* in short, all the sensations of the patient are referred to the *knee*. In *tetanus* (locked jaw), the *symptoms* pertain to the voluntary muscles, or certain of them, as those which close the jaws. The *disease* may be a *thorn* in the *foot*, or green apples in the stomach, or strichnine in the blood. In gout, the *symptoms* are in the *toe*—the *disease* is lithic acid in the blood. In rheumatism, the *symptoms* may be in the *shoulder*, the *disease* is lactic acid in the blood. I say, then, that the symptoms, even the totality of them, are not the disease, and often do not even "represent" the disease.

4th. I object to exclusive homœopathy, that it ignores the ascertained practical facts of that most common process, *inflammation*. Twenty-five years ago this would have been no great deficiency, for the whole subject was then in the most chaotic confusion; but the labors of Paget, and others, have evolved from this chaos, within a few years, a beautiful order, which is destined to work most practical improvements in our art. We do now know that this abnormal condition of inflammation may begin by a vitiation of any one of the four essential conditions of the healthy action of any part; that is, it may make its debut in the *blood* of the part, in the *blood vessels* of the part, in the nerves of the part, or in the essential structure of the part. We do *know* that when one of these four conditions is interfered with, the rest are soon involved. We do know that when the disease is grave—when the scales hang fearfully even between struggling nature on the one side, and deadly inflammation on the other—our art may come in—not to take the case out of her beneficent hands—but to aid her by removing one or more of these abnormal conditions; she taking up the work and completing the cure by the removal of the rest. Let me instance that once fearful

disease, peritonitis. We now administer the large doses of opium; we cure the nervous element of the disease (which is the leading element); we allay the horrible pain; excelling nature takes up the cure and removes the other faulty conditions. And here I know the well-informed homœopathist will meet me with the tables of Fleischmann, of Vienna: Peritonitis cases, 105; cured, 100; deaths, 5. Let me frankly and freely confess, that these tables give better results, in this disease, than the regular practice of that day (1835 to 1853); in other words, that leaving nature to fight the battle alone, is far better than to thwart her, as was done by the profession of that day in that disease. In a case of specific inflammation, where the inflammatory process begins in the blood, we antidote or eliminate the poison from the blood; nature again takes up the cure and removes the other abnormal conditions. Again: if the engorgement of the blood-vessels of the part be the leading faulty condition, we unload these by local depletion; we apply a few leeches or cups; and as before, nature, thus aided, takes up and completes the cure. All this, the now well-settled history and pathology of inflammation, homœopathy totally ignores.

5th. We say that the homœopathic diet, enjoined as a means of cure, is, like the rest of the system, fanciful and empirical; though I admit, cheerfully, that it contains some good prohibitions. Now we, of the regular profession, *aim* to adapt the diet to the condition of the patient. For instance: we take a case of consumption, where the blood is impoverished by lack of its oily element: we prescribe an apoplectic diet (so to speak), and we administer cod liver oil, and, *under favorable circumstances*, the chyle, and thence, the blood being enriched by a due proportion of oil, the general nutrition is improved (the blood being raised to the standard of health), and the patient is cured. The statistics of the Brompton Hospital, for consumptives, prove that, of all the cases treated there, in all stages of the disease, one-third are cured—the disease is arrested in another third, and the proportion of deaths is but 33 per cent. Where can homœopathy point to results like these? In these famous, and, I believe, honest tables of Fleischmann—confessedly one of the best homœopathic practitioners in Europe—all the poor consumptives (98 in number) go into the *black column*. Whole number 98; died 71, discharged uncured (sent home to die) 27; 71 and 27 is 98. If you will read Bennett, on Tuberculosis, you will find this to be the simple rationale of the matter: tubercle, from an impoverished blood, the main deficiency in the oleaginous element furnished; hence

the treatment,—and the diet is the grand part of the treatment,—should have steadily in view a *better nutrition*, a richer blood. Cod liver oil, containing the biliary principles, and hence more easily assimilable than other oils, and hence, in a majority of cases, a leading article of the treatment, unknown among the Esquimaux, while here it heads the weekly list of the City Inspector, and in New England causes one-fifth of the total mortality. No other explanation can be given than the fact of their living on a highly oily diet. The Artic explorers tell us that no *bon bons* are so acceptable to an Esquimaux belle as tallow candles. Do you ask why we see no such results in this country from the "analeptic" treatment of consumption? I will tell you. First, and chiefly, we are not sufficiently impressed with the importance of the constitution of the blood in this as in other diseases. We are too much wedded to the old idea of treating the symptions of consumption—its *complications;* too little faith in the grand fact, that it is by a *better nutrition* and a *richer blood*, and by this *alone*, that it is ever to be cured. But we see people every day taking cod liver oil and dying with consumption. True, we do; but what else are they doing or leaving undone? They are taking "cough mixtures," which destroy the appetite and prevent that *better nutrition*, from which alone a cure is to be hoped. They are shut up in close rooms, instead of inhaling God's oxygen, on foot or on horseback, in open carriages; and last, not least, they are often taking a purely *factitious article* of the oil. Take the converse class of cases—those of the *bilious* diathesis. Here the supply of oleaginous element, is in excess—the surplus must be diminished by the liver, this organ is overworked, bilious attacks result. We interdict the oleaginous and saccharine articles of diet (fats and sugars), and with or without appropriate medicinal treatment, the diathesis is overcome. One more example: Macauley (layman as he is,) has not failed to notice the marked change in the character of the diseases of England, wrought by the introduction of the *potato* and other roots and esculents, into the diet of England. When the diet of the English people consisted almost exclusively of wheat bread and meats, the albuminous group of aliments was in excess, and the result was a great prevalence of gouty and calculous diseases. When the diet of the people was changed, by the introduction of new vegetables and fruits, the gouty and calculous affections markedly diminished. Among the poorer claasses of society, those compelled to live on a poor vegetable diet, we find rheumatism taking its place; and in the workhouses of England, where, as Dickens

well satires it, "a board of fat governors spend their official time in diluting gruel and writing reports." We find that out of 860 prisoners 437, or 52 per cent., are of *scurvy;* when the wise Board of Governors aforesaid cut down the allowance of nutriment from 31 to 21 ozs. per diem. In other public institutions, where similar wise Boards of Governors undertook to keep the paupers on bone soup, we find a malignant fever breaking out and decimating the inmates two or three times over. In short, rational medicine, guided by experiment and experience, recognizes the fact, that a well-arranged dietetic scheme ought to consist of such a combination of the several groups of aliment, the *albuminous*, *saccharine* farinaceous, *oleaginous*, and gelatinous constituents, as is most appropriate to the requirements of the system of the given patient. This is of itself a great subject, and deserves more attention than it has yet received from the profession and the public. What makes *dyspepsia* so common, so universal on the highest hills in our country towns and in the most elegant avenues of the city of New York? What more than a badly regulated *diet* and a horribly unscientific cookery? You dine freely in Paris—you feel no sense of oppression. You dine freely here, and you are stupid all the evening. There, cookery is a *science;* here, it is hardly an *art.* The point I make is, that there is a science of diet that the well-educated physician appreciates and avails himself of; that the diatetic prohibitions of homœopathy are entirely unscientific and empirical; and although they may, and do, effect a great deal of good in many cases, still, like the rest of their exclusive system, they are underlied by no scientific principle, and hence incapable of universal application; but are based on a *conceit* that *beef* and *tooth powder*, *pork* and *perfumery*, may militate against the action of nux 3, or hepar 30.

And this brings me, 6th, to the ludicrous side of the argument: that medicines are efficient in infinitessimal doses; though, as plain, palpable grains of oyster-shells or camomile flowers, they are inert. On this I shall not dwell; not because it does not afford a beautiful example of the "*reductio ad absurdum*, but because its exposition, though legitimate, is somewhat stale. I only allude to it to show where the adoption of this exclusive doctrine led its illustrious founder.

*Eulogy upon M. Roux*, delivered before the Academy of Medicine, Paris, by M. DUBOIS, of Amiens. Translated for the MONTHLY, from the *Gazette Medicale de Paris.*

Philibert-Joseph Roux, Professor in the Faculty of Medicine, at Paris, member of the Institute, titulary member of the Academy of Medicine, and Surgeon-in-chief of Hotel Dieu, was born at Auxerre, on the 26th of April, 1780. His father, a surgeon, was held in well-deserved esteem in that city; where, by long and meritorious services, he had obtained the position of Surgeon-in-chief to Hotel Dieu, and the Military School.

This justly celebrated School was under the charge of the members of the learned and enlightened religious order of St. Benoit; young Philibert was admitted into it, and while there followed the instructions of the future perpetual Secretary of the Institut d'Egypte, and of the Academy of Sciences, Joseph Fourrier. Philibert was extremely dissipated as a student, but his frank and open character won all hearts, except, however, his father's, who could see nothing to hope for in the future of such a volatile and inconstant child; a mother would have been more indulgent, but our student had lost his early in life.

The Revolution was felt at Auxerre, as every where else; the military school became a national college, but the students did not on that account cease from attending upon the teachings of Fourrier; for, as their young and learned professor had taken no vows, he had only to lay aside the habit of St. Benoit, in order to resume his place among the laity: his withdrawal in those days of disorganization would have been a veritable calamity: he was competent to every demand, and successively taught mathematics, philosophy, rhetoric, and general history.

As for our future colleague, if he continued to distinguish himself, it was merely by the vivacity of his intellect and by an unequalled ardor for the sports of his age; and yet, thanks to a most happy readiness, he almost always maintained a position in the first rank among his fellow-students. But his father was none the more content; he could not believe that success thus obtained without strenuous exertions, and almost without labor, could be good metal and durable; and, therefore, believing his son's education neglected and his opportunities wasted, he considered it his duty to give up the idea, which he had always entertained, of making him a civil engineer; and obliged, to his great regret, to fall back upon his own profession, he resolved to make him at least a good and useful

surgeon, such as he was himself. In order to initiate his son in the rudiments of his art, he caused him to attend upon his own visits at Hotel Dieu, and made him perform each day what are called the minor operations of surgery.

M. Roux displayed no greater application in his surgical studies than he had shown in his literary pursuits, which did not hinder him, however, from making remarkable progress; but the quiet and monotonous life of the country town was hateful to him, and a thirst for travel began to trouble him. Young, inexperienced, eager for excitement, he seized the first opportunity which offered itself, to see other countries—not, as he did later, rolling on in a sumptuous travelling carriage, and scattering money by the wayside, but most modestly, as a health-officer of the third class, and with his knapsack on his back.

This he did, with all the resolution and gaiety of youth, towards the end of 1796. This commencement of a military career was not, indeed, either very serious or very long. Detailed at first to Andernach, and afterwards to Aix-la-Chapelle, he was discharged after eighteen months service. Finding himself thus at liberty, like an old servant, in consequence of the treaty of Campo-Formio, his first thought was to return to his own fireside. He expected to find there, dear friends and pleasant leisure; but an inexorable God watched over his domestic hearth stone: none other than his father, stern old man, who, after a few days of repose, enjoined upon him to direct his steps towards Paris, and finally to enter there upon a more serious course of study than he had followed in the past.

To bleed and to dress wounds—such was about the extent of the instruction which M. Roux had gained at the hospital of Auxerre; therefore he had to learn everything anew, and especially anatomy, which then, as now, was held in great favor at the Paris school. To this study he consecrated the first years of his sojourn in Paris; he gave himself up to it completely, and this time with so much the more zeal and success, because the young teacher to whom he had attached himself was not only a skilful anatomist, but one of the greatest physiologists that France has produced:—Bichat, who, in the wrecks of human organization, and even in the mechanism of death itself, if we may so speak, endeavored to penetrate the mysteries of life.

Yet M. Roux, the pupil of Bichat, and, in some sort, to be considered as his successor, did precisely the contrary to that which his teacher had done.

Bichat had begun with surgery; his first teachers were Marc-Antoine Petit and Desault; his first course was a course of operations, his first publications lectures and essays upon surgery. But this splendid genius soon changed his course; he gave himself up completely to physiological studies, and in this new career rapidly acquired an imperishable glory. M. Roux, on the contrary, had scarcely closed the eyes of his master, than he abandoned all physiological studies in order to devote his entire attention to surgery.

The career of the *concours* was then, as now, open to all young men of talent. M. Roux had scarcely entered upon it, when he found himself brought in opposition to a young man who was urged on by a vast ambition—a young man who was destined to be the most redoubtable and the most constant of his adversaries, whom he was thenceforth to encounter at every step; who, everywhere, and at all times, would be at hand to dispute the passage with him, and who would thus rest as a weight upon his entire destiny, up to the very moment when, snatched away by a premature death, he would leave to him, as a final burthen, his own peculiar and overwhelming succession.

It will be readily perceived that we refer to Dupuytren. For the first time, these two rivals were about to meet. Dupuytren was three years older than M. Roux; but how great a difference already between these two young men—one of whom was twenty-two years of age, and the other twenty-five!

We have seen how easily, with what freedom from care, M. Roux had passed through the first years of youth: how light had been his burden of life. Doubtless his father had imposed upon him some economy and had limited his expenditures; but this young man had had no struggle with misfortune; he had received none of the severe and strengthening lessons of adversity! Dupuytren, on the contrary, born in a small town in Haute-vienne, owed even his early education to the generosity of strangers; later, he was obliged to share, with a fellow-student, a modest chamber, furnished with a humble bed, a table and three chairs. There this earnest young man entered upon his long studies. Fate did not even accord to him that goodly gift of the gods, the friendship of a great man. And who knows whether he would have accepted it? He felt himself destined for the highest places, and to them already he aspired; he, who, in the field of science, had as yet made no conquest—he, who will have no Rubicon to cross, dared to tell his comrades that he would not be second in Rome. And what was *his* Rome? It was that sceptre of surgery

which he saw in the future, and on which already he would lay his hands.

Such was the adversary with whom M. Roux was about to measure his strength. The reward of the *concours* was the position of Assistant Surgeon in Hotel Dieu.

The victory was for a long time in doubt. M. Roux, in the oral tests, and especially in improvisations, showed himself superior to his adversary; already he exhibited that richness, that wealth of expression, which caused him to run into digressions, to return again to the subject, and then to overshoot the mark, without ever being able to keep closely to it. Dupuytren, less affluent in words, but more methodical and more severe, had the advantage in the tests, where logical reasoning and the appreciation of facts are especially required. Dupuytren was declared conqueror, and ascended that grand stage of Hotel Dieu, where he was to gain so lofty a reputation. M. Roux, however, had given evidence of such admirable talent and such extensive acquirements, that, shortly afterwards, he was appointed, by M. Frochat, prefect of the Seine, to perform the duties of acting Surgeon-in-chief of the Hospital Beaujon.

This modest establishment, situated at a distance from the latin quarter and from the tumult of the schools, seemed destined to receive those who, issuing, all crushed and bruised, from their conflicts with Dupuytren, had need of peace and quietude, to regain their wonted force. M. Roux went, therefore, to spend a few years in this quiet retreat; at a later period, the excellent Marjolin, appointed Assistant Surgeon of Hotel Dieu, was also forced to enter this asylum in search of patients to treat, pupils to instruct, and especially of manifestations of affection and kindness; all of which would have been refused him, with a superior who, not only could not admit the possibility of an equal, but who would not even permit a colleague to follow too closely.

A few years of somewhat inactive life thus passed away, while M. Roux must have struggled with his impatience, when, in 1810, he was so fortunate as to obtain an entrance into the family of a great surgeon; M. Boyer bestowed his daughter upon him, and obtained for his son-in-law the position of assistant surgeon in la Charité.

Under the guidance of such a teacher as Boyer, M. Roux might have gained what he most needed; he might have become a skilful and a wise surgeon; but for such a bold and adventurous genius as his, this guidance could only be a perpetual restraint. Boyer was his superior; for thus the regulations directed. M. Roux himself came

at length to recognize the necessity and wisdom of this subordination. He said at a later period, in strong terms, that it was a salutary authority which, when needed, could restrain the too ardent spirit of young surgeons, and that excessive boldness which springs too readily from too great independence. But in the early years of his connection with Boyer, far from recognizing and blessing this wholesome restraint, he strove against it daily, and deplored, what he called, his shackles.

There was, indeed, a strange opposition between these two characters, and these two species of talent; imagination, inventive genius, and a restless spirit seemed waging a perpetual strife with circumspection, wisdom, and good sense. M. Roux was brave, generous, and chivalric; M. Boyer was prosaical, sarcastic, but profoundly sensible; we were all witnesses of these intestine struggles; the young men rallied about M. Roux, while those of mature years were rather on M. Boyer's side; it appeared to the former that M. Roux was in surgery the image of the future, and that M. Boyer represented the past. M. Boyer considered, in fine, that surgery had reached its highest degree of perfection; that the Royal Academy of Surgery had given the finishing stroke to it, and that there was nothing more to be added. M. Roux, on the contrary, exclaimed, "No, the temple of science is not yet completed! No, our art has not attained its utmost limits!"

The death of Sabatier, occurring in 1811, had just left vacant the chair of operative surgery in the Faculty of Medicine at Paris; the lists were opened anew; and once more Dupuytren and M. Roux found themselves face to face. M. Roux was brilliant, Dupuytren profound; the former pupil of the Benedictines had a marked advantage over his competitor in Latin composition; the fruits of literary education are ever present; when the humanities have been neglected or are imperfect, they cannot be repaired, and will forever remain incomplete. However, Dupuytren was again declared conqueror, and to him was awarded the chair of Sabatier.

But it was merely a question of time in respect to M. Roux; his place was prepared in the higher grades of instruction. In 1816, by the death of Petit-Radel and of Alphonse Leroy, two other chairs were left open. In consequence of changes, that of external pathology was declared vacant; but the *concours* was suppressed, and the duty of nomination devolved upon the Faculty; the classical and popular Marjolin was put forward into the front rank, while M. Roux only attained the second place. Finally, on the 10th of February,

1820, justice was done to M. Roux; Percy having voluntarily and very opportunely resigned, he was nominated to the first rank, and on the 4th of March following his nomination was approved.

But it was not merely as an instructor that M. Roux was destined to shine; he was also to hold a distinguished rank in the learned societies. Towards the end of the very same year, 1820, he was included among the first nominations of the Royal Academy of Medicine, and became one of its most active and assiduous members. The Academy was then divided into three grand sections: M. Roux, in 1825, performed the duties of secretary to the section of Surgery; in 1827, he was its vice-president; in 1828 he was chosen to the presidental chair.

He was not, however, content to rest here. The Institute has, in the class of sciences, a section of medicine and surgery. M. Roux naturally aspired to occupy one of these chairs; in 1820, a seat became vacant in consequence of the decease of Percy, and M. Roux took his place among the number of candidates; his claims were numerous, but his eternal adversary Dupuytren had also placed himself in the ranks; the issue could not be doubtful: Dupuytren was elected by an imposing majority. In 1829, a new vacancy was declared in the section; this time, a great military star, Larrey, disputed the honor with M. Roux. For a moment the fate of M. Roux trembled in the balance; five votes only decided in favor of Larrey. Finally, on the death of M. Boyer, in 1834, M. Roux was elected on the first ballot.

We have seen that from the earliest period of his entry into the Hospital de la Charité, M. Roux had sought to distinguish himself by the importance and novelty of the operations which he wished to perform; we have shown how great was the opposition of Boyer; but there came a time when M. Roux, freed from all control, and delivered from all restraints, could fully obey all his inspirations, and listen only to the suggestions of his own genius; this was when the death of Dupuytren had opened to him the doors of Hotel Dieu.

He was at length at the head of French surgery. Up to that time, whenever he had desired to rise, he had met Dupuytren, and Dupuytren had checked his further progress.

And if, indeed, at any time he had succeeded in placing himself in the same grade with Dupuytren, it was because in that very grade there were several places: when there was but one, that one was filled by Dupuytren. There could be but one first physician to the King; to Dupuytren belonged that distinguished honor; there could

then be but one surgeon-in-chief at Hotel Dieu: Dupuytren held that post; and if M. Roux finally succeeded in obtaining it, it was because the death of his rival left him a clear field.

It was as though a final *concours* was about to open between these two surgeons; the vast establishment called Hotel Dieu was still full of the memory of Dupuytren. The shade of that great surgeon seemed still to wander in those long halls, as grave, and stern as in former times.

His aids, who are now, for the most part, distinguished practitioners, remained at their posts; when they had assembled, the spirit of their master was with them, and seemed to communicate to them somewhat of his severity, haughtiness, and disdain. In their eyes, M. Roux, compared with Dupuytren, could be but a very secondary personage.

It is said that, frightened himself at the weighty responsibilities of this succession, M. Roux hesitated a long time before accepting it; having at length decided to do so, we know how he was received, and what prejudices he had to overcome. And yet, look upon these two surgeons, Dupuytren and Roux, and determine which was the better adapted to conciliate the favor of youth; the one was a man of stern and majestic carriage, who walked with haughty and thoughtful countenance before his pupils, each one of whom uncovered at his approach and followed him in silence; the other appeared with open countenance, contented and smiling, extending to all a kindly welcome; obliging, accessible, and striving thus to swell the somewhat noisy escort, whose gayety he himself shared.

Dupuytren's literary education was far from complete; and even in his moral education there were gaps which could not be filled up; yet everything about him inspired respect and kept familiarity at a distance; his language, as well as his attitude and his manners, were simple, severe, and almost august.

M. Roux aimed at elegance, and was in every respect brilliant; doubtless there were repetitions and interminable digressions in all his addresses, but what richness of remembrances, what delicacy of analysis! And all this without gloss, without affectation, and yet with a charm, an easy freedom, and fullness of kind feeling completely inimitable.

But as the judges at this final *concours* were all indistinctly hostile to M. Roux, they decided that the language of the rash successor of Dupuytren was diffuse, prolix, full of ambiguities and circumlocutions, clogged with obscurities, with synonyms, and fine-drawn con-

clusions, while the lofty, precise, and sententious language of Dupuytren remained in their memories as a classic model of correctness, precision, and clearness.

It must be acknowledged, in truth, that in his first acts and in his manner of proceeding, M. Roux so conducted himself as apparently to justify the prejudices which existed against him. On succeeding Dupuytren he imagined, in the overwhelming ardor of his genius, that, in order to efface the remembrance of that great practitioner, he must act, and act continually. He forgot that it was neither the number nor the novelty of his operations which had raised to so high a point the reputation of his predecessor, but in fact the exquisite judgment, the certainty of diagnosis, and the rare prudence which he brought to bear on every one of his operations. It is true that he blended with these a slight degree of artifice and ostentation, and that in reality the safety of his patients perhaps caused him less uneasiness than his solicitude for his own reputation; but as, after all, these two things were inevitably allied, these minute precautions, these profound calculations, eventually turned to the advantage of the patients.

A few words more and I shall have finished the parallel already so frequently drawn between these two illustrious practitioners. It would seem that after this final *rapprochement* in the halls of Hotel Dieu, and when both have gone down to the grave, there could be no other comparison established, no further parallel carried out; there is, however, one tribunal still before which they will have to appear, which alone will pronounce upon them a final judgment. I refer to that *concours* which is opened for all celebrated men in the presence of posterity.

The history of surgery will then have to make known to the world what Dupuytren has done to extend the limits of the art, what have been his inventions, his discoveries, all his labors, in fine, and it will be seen whether or not he has left behind sufficient to justify and support that haughty supremacy, so laboriously acquired.

M. Roux, also, will be judged from the same point of view; the testimony of contemporaries will doubtless be invoked; but in the end, their services to science must rest upon their own merits. It would seem as though M. Roux himself appealed to those coming years, when he said that we may hope to live, and to live eternally, in the memory of men, if we have left behind us great works, while the talent of the professor perishes and disappears with him who possesses it.

And therefore M. Roux, more thoughtful of posterity than Dupuytren, has left to the world numerous and important works. As early as 1809, he published a volume styled *Melanges de Chirur_ie*, in which he yielded to the ruling taste of the time, and devoted himself to ingenious attempts at classification.

In 1813, he published the first part of a *Traité de Médecine Operatoire*. At a later period, when endeavoring to explain why he had not gone beyond this first part, he acknowledged that his forte did not lie in the composition of a sustained, didactic work, and still further, he regretted having published these two volumes.

Two years after, in 1815, he published the celebrated account of his *Voyage à Londres*. After a war which had for so long a time separated the two countries, and which, in respect to the sciences, had rendered them not indeed hostile, but almost entire strangers to one another, it was a happy idea to make an examination, on the very spot, into the then condition of English surgery, and to draw a parallel between it and French surgery.

I come at length to the semi-posthumous work which is the most considerable and most imposing of all those composed by M. Roux; I refer to the work entitled *Quarante ans de Pratique Chirurgicale*. In this work he is incomparable; in vain would we seek among the surgeons of the time for a writer so fascinating, of such varied powers, so cultivated and so rich in resources: he is like a great commander, who, after memorable campaigns, preserves the remembrance of all his lofty deeds in copious memoirs.

He had begun the publication of this work before 1848, but the confusion and tumult of revolutions had forced him to interrupt it; he resumed it again in more quiet times, and was laboring with unequalled activity and zeal, when, on the 27th January, 1854, he was seized with a most severe attack of cerebral congestion. This must have been an ominous warning; yet he resumed his ordinary labors, and especially the publication of his great work. Four new sheets were upon his desk; with a faltering hand he attempted to renew and strengthen his relations with posterity. We may say that he labored for posterity even in the arms of death.

A new attack supervening, arrested that courageous hand, and terminated his life on the 23d of March, 1854.

And now, gentlemen, before finishing, permit me to give my whole opinion concerning the colleague whose life I have just attempted briefly to sketch.

Three great surgeons have in some degree, filled in our eyes, the

first half of the nineteenth century: Boyer, Dupuytren, and Roux. Of these three surgeons, M. Roux was beyond all question, as an operator, the most ingenious, the most intrepid, the most daring; but to speak in strict accordance with truth, we must add, that even while giving evidence of incomparable skill, he was not always free from the grave charge of having pushed his boldness to the very verge of temerity. In him we have seen, that the qualities of a good, true, and excellent surgeon were almost all included in the art of operating with confidence and grace,—above all, with grace. More than any other M. Roux, in the course of his long career, had it in his power to be useful to the cause of humanity; he gave an impulse to science, and in many respects he extended the boundaries of the art,—would that he had had a little more of that prudence and reserve so necessary in the practice of surgery! He excelled in all, but never having imposed restraint upon himself, he did not always make the best use, either of his words or his pen, nor even of that which is above all redoubtable, the art of surgery! Eager to act, and desirous of showing himself to the best advantage, that is to say, with knife in hand, he did not always take time to assure himself whether an operation was absolutely necessary, or strictly indispensable; he merely demanded whether it was *possible;* and yet what operation is not possible to so skilful a surgeon?

Strange and brilliant nature, which erred only by reason of the very excess of his qualities; we might almost say that a jealous sprite had taken a malicious pleasure in spoiling in him the world's best gifts, by bestowing them in most prodigal measure and without discrimination; from thence came that fire, that impetuosity, which age itself could not subdue; from thence sprang that lack of order and of continuous effort, which appeared in turn in his lectures, in his writings, and in his practice. How many times have we seen him, yielding to the chance suggestion of his inspiration, commence an operation like a discourse without exactly knowing where he would stop nor how he would finish, and afterwards be himself astonished at the various steps he had taken, and the results at which he had arrived!

M. Roux was indeed a great operator, but he was so far too exclusively; he was not sufficiently impressed with that noble and indisputable truth that a surgeon, to be truly successful, must be at the same time a wise physician; that that which now constitutes the strength and honor of surgery, is the close alliance which it has formed with medicine, both in its studies and in its practice.

## REVIEWS AND BIBLIOGRAPHY.

*Poisoning by Strychnia, with comments on the Medical Evidence given at the Trial of William Palmer for the murder of John Parsons Cook.* By ALFRED S. TAYLOR, M.D., F.R.S. London: Longman, Brown, Green, Longmans & Roberts, 1856. 152 pages.

Scarcely twelve months have elapsed since the trial of Palmer for the murder of his friend Cook, by the administration of Strychnia, occupied the attention of the administrators of Law in the Old Bailey, London. The substance alleged to be employed, although known to the Chemist since 1818, yet had not been fully studied so far as its effects on animal life were concerned, and very little, indeed, was known of the changes, if any, it might undergo when in the animal body. In fact, no examination of the body of a human being with regard to the detection of Strychnia had ever been accomplished by Orfila, or by Handin. It was hence likely that there would be much difference of opinion as to the readiness with which it could be detected in the contents of the viscera. Accordingly we were prepared for some little discrepancy in the testimony, expecting that those who had least opportunity for experiment, in Toxicological examinations, would hesitate most in advancing *ex cathedrâ* opinions. Every one was aware of the difficulty experienced in separating a vegetable alkali from animal or other organic matter, whatever that alkali might be. We were surprised to find, in the verbatim Report of the Trial, one witness swearing that the fifty-thousandth part of a grain, unmixed with animal matter, could be detected—that if Strychnia had been taken in *this* case it *ought to have been found*, and that in all cases "where Strychnia had been taken in a sufficient dose to poison," *it can be detected*, and *ought to be detected*. Another Chemist swears that "of all poisons it is *the most easy* of detection."

As Professor Taylor and Dr. Rees were employed by the Crown for the examination of the contents of the viscera, and as their report showed the discovery only of antimony in these organs, although examinations had been made for prussic acid, oxalic acid, opium, morphia, strychnia, veratria, nicotina, and codeia, every one must feel an interest in reading the views of either of these gentlemen, with regard to this testimony, and on the subject in general. The former had excited the peculiar ire of Sergeant Shea, inasmuch as after having failed to detect Strychnia in the body, he nevertheless gave his opinion that the symptoms manifested by Cook, pointed to the admin-

istration of Strychnia. It was the policy of the defence to show that as Strychnia was not detected by an analyst, *therefore* it had not been the cause of Cook's death; and in pursuing this line of policy the remarkable chemical evidence of Herapeth and Letheby, already referred to, was availed of by Sergeant Shea. There is no objection to the counsel for the defence, in a criminal case, employing every honest means to obtain a favorable verdict, but we feel that attacks upon the reputation of medical witnesses for the purpose of overthrowing *their* testimony, are most unworthy the dignity of the legal profession, and merits the contempt of every good citizen. And "whatever may be the consequences to the medical jurist, although he may stand alone, and have the attacks of the public and medical press directed against him, the safety of society and the demands of public justice, require that an opinion formed deliberately after a careful consideration of all the medical circumstances, should be firmly and decidedly expressed."

Dr. Taylor first discussed the question, "*were the symptoms and appearances in the case of Cook caused by Strychnia, or might they have arisen from some natural disease?*" He answers it affirmatively for the reasons, "that they were not inconsistent with death by Strychnia, but such as from experience in other cases in the *human subject* we may commonly expect to find as a result of the action of this poison," and that they are not reconcilable with death from any other cause. One objection had been made to Taylor's opinion because the heart had been found empty, whereas in experiments on animals the heart was generally found full of blood. But upon careful examinations of the appearances presented by this organ in fatal cases of Strychnia, we find that the condition of the heart as to the presence or absence of blood varies very much. In the two cases published in this country, where Strychnia was detected, viz: the body of Mrs. Green, examined at Chicago, and that of Dr. George Gardiner, examined at Washington, the heart was found to contain no blood; and though these cases do not prove that the heart, in a case of Strychnia-poisoning, will always be found empty, yet they substantiate one thing, that such a condition of that organ is not unusual.

The second question is a most important one: *Can a person die from poison, and no poison be found, by chemical analysis, in the body?* This is a general question, and no one who is at all conversant with Medical Jurisprudence, as connected with Toxicology, would be stupid enough to say *no*. Hydrocyanic acid, even when known to be taken,

has failed to be discovered. This has also been the case with other organic poisons. The true bearing and importance of this question should be understood. If we require the discovery of the poison as the *sine quâ non* in a judicial investigation, we elevate chemistry not only to equal rank with pathology, but we depose the latter entirely from its necessary position in such an investigation. The two sciences must go hand in hand. Chemistry has accomplished much, in the last thirty years, in medico-legal investigations, but we are not prepared to draw the conclusion that it is *always* able to detect a poisonous substance, no matter what may be the condition of the body which is alleged to have contained it, or what the changes it may be exposed to in the blood. The requirements of the law are, that "there should be satisfactory evidence of death from poison," and not that the poison should be absolutely detected. The presence of the poison itself can not indeed prove that death must have been occasioned by it; but where this is corroborated by the antecedent symptoms, which themselves are the same that the poison itself, and no other substance known, usually produces, then we have the strongest ground for the opinion that it was the cause of death. Dr. Taylor cites the case of Castaign, where the prisoner was proven to have purchased tartar emetic and acetate of morphia on the same day that he had given some milk to the person alleged to have been poisoned, which was followed by vomiting and purging, inability to swallow, "stertorous breathing, a contracted pupil, a hot skin, the jaws locked, the neck rigid, the abdomen tense, and the limbs affected by spasmodic convulsions." Although the stomach and other portions of the body were examined by the best organic chemists in France,—Vauquelin, Lherminier, Magendie, Barruel, Segalas, and Pelletan,—no trace of either antimony or morphia was detected. The evidence, however, was considered sufficient; the prisoner was convicted of having administered morphia to the deceased, and was executed. But cases of a similar character are found in the records of criminal trials, where equally distinguished chemists have failed to detect the poison, and yet where other circumstances prompted a conviction of prisoners.

The third question discussed in this monograph is—*Can a person die from Strychnia, and no trace of that poison be found, by chemical analysis, in the body?* This is considered as involving the processes by which the supposed Strychnia may be separated from the solids and liquids of the body, the chemical tests to identify it as Strychnia, and the amount of confidence to be placed in the known tests, in an examination where the life of a prisoner depends on the result.

In the adoption of any of the processes which have been proposed for the removal of strychnia, the chemist will always find it safe to satisfy himself "by a preliminary trial that it, when in sufficient quantity to justify a medical opinion, might be separated and detected" by such processes. He has no right to rely upon the skill which has enabled other analysts to use a process with satisfactory results, but he must show that *he* can employ it with skill. Drs. Taylor and Rees seem to have relied upon the employment of sulphuric acid with the view of rendering the substance soluble in rectified spirit. Stas' process was not really used, nor was Handin's method availed of, both of which seem to us to be more delicate than that employed. But here we think the analysts cannot be blamed in this case, since the stomach was forwarded them for analysis, without any communication of the symptoms that had presented themselves, hence quite a series of substances were to be searched for, and we can understand how the analysts were satisfied with some methods of search that were employed, whereas, had the question been,—there is strychnia contained in the stomach, it is required to find it,—then all possible processes would have been tried if one had failed. The main point, here, is as to the accuracy of the tests,—their reliability. We know how much difficulty is involved in the separation of the organic matter from an alkaloid in these examinations. We are satisfied that Dr. Taylor has done good service to Toxicology by his statements, before the Jury, that the color-tests of Strychnia are uncertain and fallacious, *unless* we first get the Strychnia in a visible and tangible form. No Toxicologist should dare to pronounce dogmatically on the nature of a substance, which he has not obtained free from all impurities. When the crystalline form presents itself, then a delicate color-test becomes a powerful adjuvant in the formation of an opinion. Why Dr. Letheby should swear that this substance—Strychnia—is the easiest of all poisons, either mineral or vegetable, to detect, we, on this side of the water, cannot understand. We can only conjecture that the gentleman's reliance in his own analytical abilities must be vastly greater than that of his countrymen. In fact there is an amount of absurd egotism in the boastings of Letheby, Herapeth, and Nunnely on this case, that should cause their scientific evidence to be received with considerable hesitation. Why should we require less satisfactory evidence in a case of Strychnia, than in one of arsenic? And yet no chemist, with the slightest respect for his own reputation, would go into court, and state that he founded his opinion with regard to *its* presence upon the colored tests, without having tried the processes for

obtaining the pure metal. We all recognize the necessity for these confirmatory tests in the case of Arsenic, why are they less necessary in that of Strychnia? Of course we are speaking of the chemical evidence, the Toxicologist fortunately does not require so much from chemistry here, as when Arsenic is suspected to have been administered. The symptoms are *peculiar* and do not so closely resemble those of any other disease, as the effects of Arsenic do the symptoms of cholera. If chemistry can bring in evidence to support Pathology, then the Toxicologist is absolutely certain and does not hesitate. But should the former detect Strychnia, and the symptoms be altogether different from those produced by it, we think the Toxicologist would suspect fraudulent addition rather than real presence.

Taylor considers that, the "principal reasons for the non-detection of Strychnia in the body of a person who has died from its effects may depend on—

1. "*The quantity taken.* If the dose be small, from one-half to three-quarters of a grain, it may be rapidly absorbed and removed from the stomach. It is only the *surplus* of a fatal dose which is found in the stomach after death. If a man swallows a dose of ten, fifteen, or twenty grains, and dies quickly, without vomiting, then the residue, or some portion of it may be found."

2. "*On the time which has elapsed after taking away the Strychnia, until the symptoms commence.* The longer this interval, the greater the quantity of poison removed from the stomach by absorption. The poison has been found diffused through the circulation in nine minutes. If the person dies in ten or twenty minutes from the time of swallowing the Strychnia, some may be found. If he lives an hour or longer, the greater portion may be removed by absorption."

3. "*On the careful preservation of the stomach and its contents.* If the fluid or solid remaining in the stomach at the time of death is not carefully preserved, there is a great probability, if the residuary quantity be small, that it will not be found."

Dr. Taylor does not place reliance on the tests for detecting the substance in the tissues, and hence the stress laid by him upon these conditions. They hold, however, with regard to the examination of the contents of the stomach. We have not space enough at our command to go further with this portion of his monograph. Sufficient has been said to show that Strychnia *cannot always be detected*, when it has been taken, and that *even* this will not prevent a judgment in the case, since, to use the language of the Attorney General in the Palmer case—"Happily, Providence, which has placed this fatal

agent at the disposition of man, has marked its effects with *characteristic symptoms* distinguishable from those of all other agents by the eye of science."

The last topic discussed by Dr. Taylor, is the question—"*What becomes of the Strychnia in a case of poisoning in which it cannot be detected by chemical analysis?*" The counsel for the defence in this case insisted upon the statement that Strychnia, when taken, is detectable, not only in the liquids of the stomach, but even in the blood, the tissues, and probably the bones. And we think the positive statements of their witnesses, if absolutely supported—as they are not—by experience, would justify such a theory. It is due to Dr. Taylor here to state, that his evidence has been greatly misrepresented by the Journals. He has been represented as stating that to produce its effect on the vital organism, it must undergo such changes that would alter its entire nature, and hence prevent its detection. In fact he made no such statement. He had adopted the view advanced by Baron Liebig, that the alkaloids undergo "partial though not complete metamorphosis in the blood." In answer to the question—whether Strychnia undergoes decomposition as it mixes itself with the organic tissues—the reply was, "I believe it *partially* undergoes some change in the blood." The gentleman did not support the theory that the gastric juice or the processes of decomposition would *necessarily* destroy the substance. We do not see the necessity for admitting Liebig's theory in order to account for the failure of its detection. When a *minimum* dose has been taken, the poison *may be so distributed through the blood and tissues* that it would present difficulties almost insurmountable in obtaining a *sufficient* quantity to furnish the crystalline forms, and the colors which the peculiar tests produce with this substance. This fact is sufficient, without having recourse to a theory, which has not, as yet, been fully established.

Space forbids us examining any further this interesting monograph, which we recommend as a capital resumè of the whole subject. Its republication in this country, so as to give a wider circulation than the English edition can have, would be supplying the Toxologist, the Chemist, and the Physician with a most excellent and valuable treatise on the subject,—the most valuable of all yet published on the practical difficulties which arise in all examinations of Strychnia-poisoning.

L. H. S.

*Annual Report of the Commissioners of Emigration for the State of New York, for the year ending December* 31, 1856. New York. 1857.

*Annual Report of the Physician-in-chief of the Marine Hospital, at Quarantine.* Presented to the Legislature February 4, 1857. Albany, 1857.

These reports, which in ordinary years are extremely interesting to the statistician, and, in a sanitary point of view, are especially so this year, from the fact that they contain a history of yellow fever, as it prevailed upon the shores of our bay during the last Summer.

The first is the complete report of the Commissioners of Emigration of all the institutions which fall under their charge, including the Marine Hospital and Quarantine establishment at Staten Island ; the Emigrant Refuge and Hospital at Ward's Island, and the Landing Place for Emigrant Passengers at Castle Garden.

The number of alien emigrants who arrived at this port in 1856, was 142,342, much less than in 1854, but more than in 1855. The number of patients received at the Marine Hospital at Quarantine, affected with contagious or infectious diseases, was also much below the average of former years.

In the hospitals at Ward's Island, requiring medical aid and surgical treatment, there were 6,147 cases treated in 1856, against 11,532 in 1855, falling much below the average of former years. Of this number admitted into the hospitals (which includes 776 remaining from the previous year and 406 births) 388 died. There were also treated in the Refuge department 5,131 cases not demanding regular hospital treatment and attendance, of whom 138, many of them old chronic cases, died.

The percentage of mortality in the Hospitals proper was, for 1856, 6.31 per cent. on all cases treated, and calculated on the discharges, 7.32 per cent.

On all the cases under treatment in Hospital and Refuge (making in all 11,278), the percentage of mortality was 4.66.

In the Surgical department the whole number of cases treated were 1,756 ; the number of deaths 53, or about 3 per cent. of the number of cases treated.

These statistics, which we have taken from the Report, will compare most favorably with any former year, or any institution of a similar character.

From the Annual Report of the Physician to the Marine Hospital, we gather some important data upon subjects which affect the institution of quarantines, and from this Report, also, we get a detailed

account of the history of yellow fever as it fell under the immediate attention of Dr. Harris, the physician of the Hospital.

The total number of patients remaining from the previous year and received into the Hospital during the year, was 1,648, of whom 203 died, and 1,368 were discharged.

The principal diseases treated to which Dr. Harris has especially drawn attention in the Report, are Typhus Fever, Small Pox, Asiatic Cholera, Remittent Fever, and Yellow Fever.

Owing to the excellent sanitary conditions of the Hospital, the mortality from all these diseases, with the exception of the latter, was quite small.

To the latter disease Dr. Harris has given especial attention. In this Report he has stated its period of irruption, traced its progress in the Hospital, the quarantine grounds, and along the shores of the bay, on both Staten and Long Islands, and by means of a map which accompanies the report, the extent of the infected district, and the position in the bay of the quarantine and infected vessels is shown.

Very few persons are at all acquainted with the extent of the fever of last Summer, or the severity of the attack.

In the tabulated summary, "we have recorded," says Dr. Harris, "five hundred and thirty-five cases, and adding three cases that occurred among patients convalescent from other diseases, in the Marine Hospital, we find the total number of well-authenticated cases of yellow fever occurring in the various localities in the vicinity of the port of New York during the past Summer and Autumn, as ascertained by the most rigid investigation, was *five hundred and thirty-eight*. More than one-third of this number died of black vomit."

The lesson which Dr. Harris draws from a reviewal of the facts given in this Report upon the subject of the removal of the Quarantine establishment from its present locality is, "that the present Quarantine anchorage is most favorably located for the ready diffusion of the infection of yellow fever to the populous regions adjacent to the waters of the port and vicinity of New York." The authorities of the State and city have a weighty responsibility if they recklessly neglect the teachings which are contained within the tables of this excellent Report.

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### *Gregory's Chemistry again!*

Since our notice of the American edition of this book, in the MONTHLY, our attention has for the first time been more particularly

directed to the Fourth English Edition of the Organic portion, which we had not seen before ; and although we had made the statement that "the volume on Organic Chemistry is the best that has appeared for the medical student," we are somewhat surprised to find that the English edition is immensely *the superior* of the American edition, which purports to be its reproduction. We were delighted with the clear and intelligible manner adopted by Gregory, in the treatment of this abstruse branch, it was so superior to all other text-books. The fact was *not* overlooked by us, that the American edition was *not fully up* with the state of the science; yet it seemed that this would be corrected in another edition. We are surprised to find that the editor has palmed off the edition of 1847 as the *last* English edition, while the Fourth English Edition is unexceptionable and as complete as a treatise on this subject can be. So, that while our commendations have been given to *as much* of the English work as the American public were supplied with, we must, nevertheless, *now* condemn the editor for *not* giving us a reprint of the *best* edition that the author has published. L. H. S.

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## PROCEEDINGS OF SOCIETIES.

### NEW YORK MEDICO-CHIRURGICAL SOCIETY.

[Reported for the American Medical Monthly by J. O. Bronson, M.D., Secretary.]

September 23d, 1856.—*Dr. Thrasher* presented a very beautiful specimen of atheroma, exhibited throughout the arteries of the brain, accompanied by the following history:

Hannah Jackson, colored, sixty years of age, entered the Colored Home Hospital, August 24th, 1855. She had in her day been addicted to intemperance, debauchery, and sundry other vices not in accordance with the rules of well-regulated society.

Her disease was entered upon the Hospital books as dementia, and, as far as I could learn, from that time till her death, manifested the ordinary symptoms of that affection, gradually becoming more and more stupid and feeble. At times she was quite unruly, and once even attempted to commit suicide. At the time I first saw her, which was about three weeks before her death, she kept her bed the greater part of the time, though able to walk about. Paid no regard to cleanliness or decency in defecation and micturition—sometimes

would answer questions, and at others would not—stared vacantly about with a peculiar blank expression.

Had a varicose ulcer upon her leg, and several bed sores, of which she took little notice.

She continued thus till Wednesday morning, September 18th, having been in this condition more than a year, when she became comatose—pulse slow and feeble, and the action of the heart very labored. In this condition she lingered till Thursday evening.

*Autopsy.*—Eighteen hours after death; body considerably emaciated; cadaveric rigidity imperfectly established. Brain of a pale color, slightly atrophied, and considerably softened, presenting in general an unhealthy appearance. The ventricles were found distended with a serous fluid—a few drachms were also found in the cavity of the dura mater. The arteries were found extensively ossified at the base of the cranium, especially at their entrance through the foramina, and more or less ossific or calcareous deposit was found throughout their whole extent. The coats of the arteries were unusually firm and thick, as will be seen from the accompanying specimen. They contained very little blood. The pituitary body seemed to be nearly destroyed, and the cella Turcica bore strong marks of chronic inflammation. While the dura mater all about was smooth and shining, just at this point it was disorganized, and its fibre, together with the investing membranes of the pituitary body, were infiltrated with a turbid bloody serum or sanies, filling up the whole cavity and seemingly extending to the bone beneath. Each of the lateral ventricles, attached to the choroid plexus, and the third ventricle also contained a small tumor or coagulum, the size of a pea or larger, of a gelatinous consistence, semi-transparent, and of a light color, resembling coagulated fibrin, interlaced with vessels.—Thorax: Lungs healthy. Heart slightly hypertrophied on the left side; walls of the right ventricle uncommonly thin. General tissue of the heart flabby and easily torn. Slight atheromatous deposit in the coronary arteries and in the aorta. All the arteries were thickened and firm—the arch of the aorta almost cartilaginous. Abdominal organs normal.

*Dr. Conant* made some observations on the occurrence of apoplexy without disease of the arteries, and remarked that it was very rare for so much disease to exist in other arteries and not in the aorta. He also remarked upon the usual condition of the skull as being very thick in those persons in whom the brain was found condensed and atrophied, and instanced cases coming under observation in making

*post mortem* examinations at the Asylum for the Insane, at South Boston, Mass. He had thought that the skull had become thick to compensate for the loss of substance in the brain.

*Dr. Cox* also remarked that he had observed a similar condition in like cases.

*Dr. Bronson* exhibited about two ounces of fluid loaded with floculi and amorphous matter collected from a case of chronic synovitis, by incision into the cavity of the joint. The patient suffering from the disease, a man about sixty-five years of age, and a ship carpenter by trade, about four years ago, while carrying a burden in his arms, fell and sprained his wrists. A few days after, while rowing a boat against a strong tide and wind, he was obliged to make great exertion, producing most painful effects. As the night came on the pain became so severe that he could not sleep. The wrist continued weak and more or less painful for three years. About one year ago, a swelling appeared upon the back of the joint, as near as could be ascertained, like a ganglion. From that time the swelling increased gradually until at the time of making the incision, the wrist had become about four inches in its radio-ulna diameter, and extended from the carpo-metacarpal articulation a distance of five inches toward the elbow. At a point about one inch above the styloid process of the ulna, the most prominent point of the enlargement, presented and gave a sense of fluctuation. At this point an exploring trocar was passed, and a few drops of colored albuminous fluid flowed. After withdrawing the trocar, an incision with a bistoury was made about one-half inch in length, and the fluid presented was collected. The solid bodies in the fluid, being subjected to the microscope, presented a granular amorphous appearance. The fluid was stained with blood, and had floating in it some few pus globules. Dr. Bronson anticipated a favorable result, although the patient's age and the circumstances bore unfavorably upon the issue.

*Dr. Budd*, upon request, then gave a brief review of his observations on yellow fever, at Bay Ridge, and also a synopsis of his own case.

About the first of August he was called to see a case in consultation with Dr. Crane, in the person of the eldest daughter of a gentleman living at Bay Ridge. The disease rapidly ran its course and the patient died in three days. In a few days the second daughter was attacked and Dr. Budd was again called in consultation. Her symptoms were nausea, intense pain in her back and head, suffusion of her eyes, and in her skin a yellow discoloration. The

case was comparatively mild. She was placed under the mercurial treatment, and the peculiar effect was produced upon the gums.

The father was then attacked with very violent symptoms. Mercury was administered and ptyalism effected on the second day, yet he died on the third day with black vomit. Suppression of urine was observed through the whole course in all three cases.

At this period of time the disease began to spread from the Bay to Fort Hamilton; Dr. Crane having some ten or twelve cases under his care. In three cases, quinine was given, as recommended by Dr. Stone, and they died of cerebral congestion. For three weeks time he visited the infected district daily. As to contagion in the disease the Doctor was inclined to speak negatively. Out of nine attacked, who were removed out of the district, five died. In the convalescent stage, rest is most certainly to be enjoyed. A young man, so far recovered as to be able to dress himself, rode four or five miles, and died the next day of hæmorrhage from the stomach. Hæmorrhages were general. The mineral acids were serviceable. The muriated tincture of iron was valuable.

As regarded his own case, it was first exhibited in a sense of weakness, while returning home after visiting some patients with Dr. Crane. On arriving at his house he had pain in his head, and in five or six hours, severe pain in his back, and he became somewhat delirious. Dry cups were applied. The next day nausea appeared, which continued for a week. A blister was then applied over his abdomen, and upon the blistered surface morphine was sprinkled. This, and only this, was effectual in relieving the symptoms of pain and nausea. For twenty-four hours he had retention of urine. When it flowed it was scanty, dark colored, and highly offensive. When convalescence commenced his pulse was as low as forty-five. He was confined to his bed three weeks.

October 14th, 1856. *Dr. Conant* presented, for *Dr. C. A. Budd*, a lung, the kidneys, and a portion of liver, accompanied by the following history:

The subject from whom the accompanying specimens were taken, was a gentleman 38 years of age, who had always been a high liver, and an habitual drinker,—seldom intoxicated, but occasionally drinking to excess,—and of irregular habits. He had, for several years, been subject to attacks of dyspepsia, usually following excess, which were always relieved by a brisk cathartic. About two years ago, he began to complain of urinary difficulty, passing very large quantities of water. This he attributed to an obstinate attack of gonorrhœa,

which had existed some eight or ten years previously. Suspecting diabetes, I had some of his urine analyzed, and it was found to be unusually rich in sugar. An animal diet, with total abstinence from all farinacious compounds, and vegetables, was enjoined, and the tr. ferri muriat, with opium, prescribed. He continued on this course of treatment for a long time, with marked and decided benefit; but latterly,—considering himself well,—he relapsed into his old habits, and on Sunday afternoon, the 12th inst., (having attended a wedding on Friday night, and indulged freely, and feeling slightly unwell all day on Saturday,) he was attacked with severe abdominal pains, which he could refer to no particular locality. From this,—resisting all treatment—he rapidly sank into a state of collapse, and died on Monday morning, with tetanic spasms of the opisthotonic character.

Autopsy eight hours after death: The rigor mortis well marked, and the animal heat still existing. The intestines congested, as well as the mucous lining of the stomach, which was also slightly softened. The liver bore strong evidences of what is commonly known as the "rum liver," and was considerably softened and enlarged--under the microscope it showed fatty degeneration in an advanced stage. The kidneys were very much hypertrophied and bore evidences of Bright's disease. Microscopic examination confirmed the appearance. The heart was healthy, and at the summit of the right lung (as may be seen) was found a calcareous deposit of considerable size. Another peculiar condition of this viscus may be observed in the fact of there being six well marked lobes. The urinary bladder contained about ℥vi of urine, which, strange to say, chemical analysis showed to be free from sugar, but loaded with albumen. Brain not examined.

*Dr. Conant* made some remarks on the pathology of diabetes, and asked if patients ever recovered from the disease?

*Dr. Cox* replied that they did, and instanced a case in support of the declaration.

The calcareous deposit in the lung elicited remarks from *Drs. Davis, Richards,* and *Lee,* concerning its pathology and frequency *Dr. Richards* related a case in which calcareous deposit was expectorated, presenting casts of the minute bronchi. *Dr. Cox* spoke of a *post mortem* case, which exhibited the deposit in a state of mortar, still soft.

*Dr. Cox* presented, for *Dr. Carnochan,* an amputated hand, exhibiting a tumor, of malignant character, in its palmar aspect, and a leg amputated just above the knee-joint, for chronic synovitis. The tumor first appeared twelve years ago, at that time about the

size of a walnut. The man being in the constant habit of using a sledge-hammer, kept up thereby a continual irritation, and it gradually increased, until it reached its present size of about two and half inches in diameter. The microscope confirmed the opinion expressed concerning its character. As far as can be ascertained, the case is original, there being no hereditary taint.

The leg was removed that day from a woman, who was received into the Hospital on the first of June, then very feeble and suffering extremely from the irritation produced by the long existing synovitis. Amputation was at that time proposed, but not performed, on account of the opposition of the patient. No amelioration following treatment, she finally consented to the removal of the limb. Upon opening the joint, extensive disease was exhibited, it being the seat of a scrofulous abscess; the bones contiguous were denuded of periosteum.

*Dr. Thrasher* presented a *kidney, stomach, spleen,* and portions of *other viscera,* removed from a patient deceased from *albuminuria,* with the following account of the *autopsy,* made fourteen hours after death: Cadaveric rigidity quite firm. General serous effusion throughout the areola tissue of the trunk, but not excessive. Jugulars distended with blood, and prominent. Body not emaciated, but covered in many parts by a layer of adipose tissue. On cutting through the integument over the abdomen and thorax, a serous fluid flowed very profusely, and blood, of a watery character, flowed from the veins where they were divided. The right kidney was of about three-fourths the normal size, invested by very loose cellular tissue, between it and the peritoneum, which was everywhere attached to it loosely. The color was nearly natural, though a few yellowish patches were seen upon its surface. The surface was covered with minute sacs in the cortical substance of the organ and not projecting beyond the surface. These were filled mostly with a clear transparent fluid—some seemed to contain air or gas; they were from one to three lines in diameter. There were three or four cysts of apparently the same character, though very much larger, and in every case projecting beyond the plane of the surface of the organ, which contained a dark fluid—one of these projects nearly the size of a filbert, and seemed to contain a dark brown fluid. The left kidney was larger, but presented the same encysted appearance; but none were so large as in the right, and none contained the dark matter. Now, did this man die of uræmic poisoning, effusion into the pericardium, or organic disease of the heart? I tested some of the blood and serum from the thorax,

but did not detect any urea—at least any odor of urea. I had no means of examining in any other way at hand. *Thorax*—effusion in the thoracic cavity, very slight. Right lung healthy in appearance, very much enlarged, and occupying rather more than half the thoracic cavity; it collapsed but slightly on the admission of air. Left lung atrophied—occupying only about one-half the left pleural cavity, and firmly aherent to the ribs for about the posterior third of their length, also firmly to the diaphragm below—a portion of the lower lobe seeming to be enclosed in a fibrous investment, projecting from the diaphragm. The lung tissue was of a dark color and very firm. The heart and distended pericardium occupied the remaining half of the left plural cavity. The pericardium contained from a pint and a half to a quart of clear fluid. The heart was greatly hypertrophied—of nearly twice the normal size. The right ventricle and left auricle were each distended with uncoagulated blood. The left auricle was enlarged, so as to form an aneurismal sac capable of containing ℥viij or ℥x of blood. The walls of the left ventricle were greatly thickened; the left side of the organ, measuring from the apex to the base of the auricle, was about an inch shorter than the right. The left ventricle contained a small clot, the only coagulated blood which we saw during the examination. Both sides of the heart contained a large amount of coagulated fibrin. In cutting through the inferior vena cava, we discovered a mass of clotted fibrin, and on taking hold of it, drew out a cast of the vena cava, hepatic, iliac, and femoral veins; no such coagulum was found in the aorta.

*Abdomen*—liver about the normal size, but presented the tuberculated appearance of cirrhosis; extremity of the left lobe attached to the diaphragm. The spleen was found, surrounded by a fibrous cyst, lying against the stomach, near its cardiac orifice, to which it was firmly attached; it was also attached to the diaphragm opposite the attachment of the lung before spoken of. The left extremity of the pancreas was also adherent to the spleen. On opening the cavity of the stomach, a cicatrix was found opposite to the attachment of the spleen, which led us to conclude that the abscess, from which he suffered some two years ago, was an abscess of the spleen, opening into the stomach, and the displacement and adhesions were also the result of it. The *pancreas* was enlarged and hardened, and the right extremity was attached to the duodenum.

## SELECTIONS.

*The treatment of Ovarian Disease* has lately occupied the attention of several meetings of the Academy of Medicine in Paris. We have already given the translation in full, of the observations of some of those who participated in that discussion. The following epitome of the more important conclusions of other members of the Academy, we take from the *Edinburgh Medical Journal*, for March, 1857.

*M. Huguier*, again, after dilating at considerable length on this subject, comes to the following conclusions :—

1st. Ovarian cysts are not of so benign a nature as is generally supposed, but, on the contrary, they are, on many accounts, of very serious importance.

2d It is an error to suppose that, in the majority of such instances, life is prolonged to an advanced age : the younger the patient, the more speedy is the fatal termination.

3d. Unilocular cysts, which have not undergone any organic alteration in their parietes, and with serous, sero-sanguineous or albuminous contents ; cysts which originate in extra-uterine pregnancy ; and purulent cysts, are those in which a cure is most easily obtained.

4th. Areolar and multilocular cysts are not to be interfered with, unless, while being of small size, they occasion much suffering or inconvenience ; in such cases there is less risk in adopting the ordinary surgical treatment than where they exist of larger dimensions.

5th. The most favorable period for operating is, when the tumor is not yet very large, but has begun to occasion suffering to the patient, or to give rise to the disturbance of function.

6th. The employment of only one mode of treatment seldom succeeds ; most frequently several must be combined.

7th. Among injections, those of iodine appear to be employed with most success ; they are seldomest followed by bad consequences, and are most likely to prevent purulent infection.

8th. The cyst should, as far as possible, be acted upon through the vagina, rather than through the abdominal walls.—*L'union Médicale*, Nov. 13, 1856.

*M. Jobert* (de Lamballe), after advancing his views upon this subject, stated, as a *resumé* of his remarks, that, in his opinion, the pathological anatomy of ovarian tumors ought not to be taken as the guide for their therapeutical treatment, since it is, in all cases, upon the cystic membrane that injections act in determining the effusion of plastic material and the occurrence of adhesion. Whether the cyst may have originated from a vesicle, from a pyogenic membrane, or from a sero-mucous sac, it will be found susceptible of obliteration and cure, without having suffered any other change or degeneration.

Punctures, repeated at short intervals, may produce obliteration of the cyst by an albuminous deposit; and the mode of cure by injection, he regards as similar to what takes place in hydrocele under the same treatment.

Cysts of long standing, and which have attained a large size, although they be not cured by injections, are yet capable of being beneficially modified by them, inasmuch as the quantity of exudation is diminished under their use. Injections of iodine, or alcoholic injections, are sufficient to produce a greater or less degree of inflammation in the cyst, when brought into direct contact with its internal surface; and such injections, made into the liquid matter contained in the cysts, never occasion any accident, while they, at the same time, produce adhesive action. The tumor may reappear after its obliteration, and a repetition of injections seems necessary for the complete cure of ovarian cysts.

Simple puncture, says M. Jobert, should be preferred, when the cysts are much inflamed and degenerated. It ought to be early performed, in order to avoid the risks of morbid changes in the cyst, its rupture, diffuse peritonitis, acute or chronic, and exhaustion of the patient's strength.

Incision he considers necessary in some exceptional cases, such as those of circumscribed abscess in multilocular cysts.

Electricity, M. Jobert also mentioned as capable of exciting absorption in some multilocular cysts; and the extirpation of ovarian tumors, he concludes, by characterizing as an operation dangerous in its nature, and one which ought to be resorted to only in very few cases.—*L'union Médicale*, Nov. 20, 1856.

*M. Velpeau*, in addressing the Society, said, that he questioned whether the duration of ovarian cysts could with certainty be determined. The views entertained upon this subject were far from being so clear as might be desired—the statistics collected with reference to it setting forth ten years, six years, and two years, as the average duration of the disease. But an important point to be ascertained was, upon what grounds were these statistics founded. It must be kept in mind, that, so long as a cyst of this nature does not exceed the dimensions of an egg, or even an orange, we never know of its existence; and where a medical man has a tumor thrust upon his observation, it may be of the size of a child's head, far less a sac capable of containing ten or twenty litres of fluid, the date of its commencement is perhaps eight or ten years back. We therefore do not, and cannot, know what is the average duration of this disease.

I believe, says M. Velpeau, that, in the great majority of cases, patients live longer than six years, perhaps seven or eight years, after these tumors become appreciable; and as, in many instances, without any treatment, life is prolonged even for fifteen or eighteen years, it would be unwarrantable to adopt any measures which involve great danger in themselves; yet, at the same time, since, sooner or later, this disease leads to a fatal termination, there is

evidently room, in one sense, for operative interference. With regard to the pharmaceutic treatment of the affection, it appears somewhat remarkable that MM. Cruveilhier and Trousseau should deny the possibility of cure in this manner, as I am confident, says M. Velpeau, of having seen such cures in my own practice, although it may be impossible for me to convince others of this circumstance.

When the cysts are of small size, their spontaneous or accidental rupture may, as has been stated, lead to their cure; but, in many cases, death is also a frequent consequence. According to statistics given by Tilt, in seventy cases of rupture, thirty deaths resulted—a proportion which does not make such a mode of cure very desirable. Besides, it is not a termination of frequent occurrence. I have seen it happen on two different occasions, and in both with fatal effects.

I come now to speak of simple puncture—palliative puncture—a remedial measure resorted to on all hands. I have observed, with regret, that a certain passage in my first address to the Academy, should have given rise to some very exaggerated notions of the serious nature of this operation. M. Trousseau has referred to the table of twenty-one cases of simple puncture, as they have been drawn up by Southam. Among these cases, four, it appears, died in twenty-four hours, three in the course of the first month, and fourteen at the end of a year. I cannot admit this proportion, although similar mortality, and even greater, is found among the statistics of several English and German practitioners. Thus, in thirty-six cases given by Lee, we find three women dead within twenty-four hours after the puncture was made, six in some days, and twelve in a year; in short, twenty-four deaths during the course of the first twelve months. The statistics of Kiwisch are not less dreadful: of sixty-four cases of puncture, we find nine women dying within twenty-four hours, and others in less than a year. Among 132 cases cited by Fock, we find seventy-four deaths in less than a year. The four cases in which I myself have seen death follow closely upon the operation of puncture—and it was only in four cases—do not rank among ordinary instances. In upwards of thirty years' practice, I have punctured 310 or 312 times, and according to my notes and recollection, the women have survived from six to eighteen years. One of them, on whom I have repeated the operation thirty-eight times, has lived for fourteen years.

How, then, are those alarming statistics to be explained? Simply by their being framed under different circumstances. Favorable in the hands of one man, they become unfavorable in the hands of others; for example, if puncture is performed *in extremis*, the operation may be speedily followed by death. But I need not dwell longer upon the reason of the discrepancies existing between the results obtained by our brethren of England and Germany, and those every day falling under our own observation. In my own opinion, the operation of palliative puncture is not dangerous, except in certain complicated cases. If death occasionally supervene upon

its performance, it is only in the same way that we find bloodletting occasionally followed by phlebitis.

With reference to the radical cure by extirpation, there is an admirable paper by M. Ch. Bernard, physician to the hospitals, published in the *Archives* of this year, in which I observe a table as follows: of 292 cases of extirpation of the ovary, 100 deaths, and 100 cures; ninety-two incomplete operations, and of these thirty terminating fatally. Now, I ask what is meant by the operation being incomplete? It is this: the abdomen being laid open to an extent proportioned to the volume of the cyst, adhesions being discovered, which render the removal of the tumor impossible—this wound is brought together again,—and—thirty women in ninety-one die after such an operation, while simple puncture kills twenty-one in forty! Another calculation, cited by M. Bernard, gives seven deaths in thirteen cases of ovariotomy; and a third gives fifteen in fifty. Such results I can scarcely comprehend. We consider a a small opening made in the peritoneum, as is done in strangulated hernia, a most serious matter; while it would seem to be a simple operation to lay open the whole abdomen, to dissect the peritoneum, as if for an anatomical demonstration, and to remove a tumor, in doing which, too, we require to employ such a number of ligatures. The fact is somewhat remarkable. Can it be that unsuccessful cases are hushed up, and that those published as successful ones, are not so in reality. Whatever it may be, let us keep this in mind, that fifteen women did die in consequence of the *incomplete* operation; and, had the cyst in these cases not been interfered with at all, in all probability the patients would have lived out the ordinary term of those laboring under this disease.

I would, therefore, altogether discountenance the performance of extirpation, although I rather inclined to advocate it in 1839; to render it justifiable, life would require to be threatened, and to render it rationally practicable, the tumor would require to be of small size; but then, in this condition, it would not be inconsistent with the duration of life for many years. We must not envy our American brethren their practice of ovariotomy: surgery in France is at the present day in an excellent position, at once combining boldness and caution, while it appears to have renounced all rash operations; wherever it interferes, it is desirous that the operation should be less dangerous than the disease.

The next subject to which I would refer, is that of puncture, followed by irritating injections into the cysts in the ovary. This method is not in every respect new, as it is similar to that followed in the treatment of hydrocele. Towards the close of the last and the commencement of the present century, the surgeons of England and France have occasionally injected hot wine into cysts of the abdomen, and even into the peritoneum itself, and it is now thirty years since M. Jobert has injected alcohol into ovarian tumors. How is it, then, that such an operation has, at the present day, created such an excitement? It arises from the bad consequences

which followed those former trials of it—from the number of deaths which occurred as their result. But this state of matters has been materially altered, since; from innumerable examples, I demonstrated that injections of iodine, in the case of hydrocele, determined an inflammation of the tunica vaginalis, less painful in its nature, and of a moderate degree of intensity; that a portion of the liquid injected might be retained with impunity, and that it was even advantageous to let it be so; and that, should a few drops of the iodized fluid escape, it did no great harm. Moreover, I also observed that iodine did not give rise to inflammation, except where it came in direct contact with the tissues; thus, hot wine could never be risked as an injection in congenital hydrocele, lest, as there was good reason to fear, a spreading inflammation should be set up, and which might extend by contiguity to the peritoneum. Such inflammation is so little to be dreaded when we employ a watery solution of iodine *(de l'eau iodée)*, that, in injecting the sac of a hydrocele which communicates with the cavity of the abdomen, it is scarcely necessary to made any separation for the moment, between the two cavities by pressure of the finger at the inguinal canal. Having observed this property in iodine, I have used it with success in cysts of the spermatic cord, in old hernial sacs, for the radical cure of hernia, in cysts of the groin, of the thigh, of the axilla, of the breast, and of the thyroid; I have also employed it in cases of obstinate articular effusions, and principally in that of the kee—the most extensive articulation in the body. The same experiments were made at that time by M. Boinet and many other surgeons. The application of this mode of treatment to ovarian cysts is pointed out in my treatise on operations at the date of 1839; and, in fact, numerous cases of that affection have been already treated in this way. M. Robert appears to be the first who attempted it in France, and others have introduced it in England and Germany. M. Boinet has practised a great number of iodine injections, at the same time that I have also tried it in a number of cases, although with less enthusiasm than our confrere, who has followed out this method so vigorously as almost to make it his own. MM. Jobert, Monod, Demarquay, Huguier, Fock, Briquet, and Nelaton, have all reported their quota of facts; the whole, along with my own cases, forming an assemblage of 130 examples. We must, no doubt, cut down this number, for some cases have been doubly mentioned in the statistics of M. Briquet, for instance, and M. Boinet. I cannot say exactly if, among the examples of MM. Monod and Demarquay, any of these are introduced into the statistics of each of these surgeons. M. Jobert speaks of twenty-six operations, and, in his address of Tuesday last, he only detailed ten cases. Could it be that these twenty-six operations were performed on only ten patients? In all, there have been thirty deaths, and sixty-four cures; in the other cases the disease recurred. In 130 patients, thirty deaths is a considerable proportion; should such really be the mortality of the operation, I would not be inclined to recommend the injection of iodine in ovarian cysts.

On the other hand, the proportion of sixty-four recoveries is worthy of being taken into consideration.

But to what cause are we to attribute the unhappy termination of some of these cases where iodine has been injected? Ovarian cysts differ considerably from each other; the diagnosis of these, according to certain of our learned colleagues, is an easy matter. Perhaps so; but, for my own part, I regret being less competent in this respect. In my opinion, nothing is more difficult. There are cysts of the Wolffian bodies; cysts upon which MM. Follin and Verneuil have published interesting memoirs; there are some of these singular cysts attached to the ligaments, like the nest of a loriot to the branches of a tree; some are connected with the uterus, and of these M. Haguier has given an excellent description; several enclose a thick epithelium, which prevents the adhesion of their parietes by inflammatory action; and, among the cysts proper to the ovary, those arising from a Graafian vesicle contain a gelatinous or a sanguineous fluid, while the contents of the others are serous. The cysts of the peritoneum, like these last mentioned ovarian cysts, resemble hydrocele in their anatomical constitution, and require the same treatment. But it seems to me difficult to establish between these different kinds of cysts any positive diagnosis, and yet it would be of importance if we could ascertain the nature of the fluid contained within their sacks. I have already mentioned, that when the contents are serous, it indicates that the surface from which it is effused is serous also, and that it is in these cases that the chances are most favorable for the operation. It may be asked, whether it is then to be adopted exclusively in cysts of this nature; but, having seen it succeed in those where the contents were bloody, I believe it also suitable for cysts enclosing fatty or sanguineous matters.

Of thirty cases of death, however, to which I have just alluded, twenty of them do not belong to what is more correctly the iodine injection method, but to a mode of treatment under consideration at the present day, and which consists in leaving the canula of the trocar within the wound. The operation of puncture, followed by injection, has been erroneously confounded with that where a foreign body is left in a permanent opening into the tumor. But the circumstances of the two cases are entirely different. In the first, the curative process takes place under the skin. It is a subcutaneous method of cure, and is unattended by suppuration. In the second, suppuration is induced; and, supposing this to take place over the surface of a sac, capable of containing thirty litres, how is the patient to withstand such a secretion of pus? The iodine injections practised in such cases, moderate the amount, and restore the quality of the pus secreted, and thus do not occasion exhaustion by the drain produced. Moreover, the facts speak for themselves. On twenty occasions, and perhaps oftener, death has followed a permanent opening being made in these cysts, as the result of cases by MM. Robert, Briquet, and Fock, has proved; and now, if we deduct

from the thirty cases of death, twenty resulting from puncture where a canula or sound was left in the wound, there remains only ten deaths to sixty-four cures. There is here a very satisfactory proportion, and one which may in future become still more so, since as yet this method of treatment has been employed more as an experiment than otherwise.

The essential point to be established is, whether puncture, followed by iodine injections, is attended with greater danger than puncture alone, or are both methods equally safe? Should the latter conclusion be arrived at, then there is no difficulty in the choice. We have in the iodine injections a mode of treatment by which three patients in four are cured—that is to say, a most serviceable remedy. Science, as well as humanity, are equally interested in the early decision of this question.

In conclusion, it appears to me, from what I have seen and from what has been observed by others,—

1st. That ovarian cysts have a duration of six, ten, or twelve years.

2d. That they are susceptible of cure, either spontaneously or under the influence of pharmaceutic treatment.

3d. That a cure may follow their rupture, although such an event is more generally attended with unhappy consequences.

4th. That palliative puncture of ovarian cysts is not a dangerous operation; that it may lead to a complete cure in certain cases, these being indeed very rare; that it involves the disadvantage of inducing, or hastening the exhaustion of the system, by occasioning the loss of enormous quantities of fluid.

5th. That extirpation is an operation so terrible in its nature that it should be proscribed, even allowing the alleged successful cases of its performance really to exist.

6th. That the only injections, of an irritating nature, to be employed, are those of iodine; and that these are applicable in all the serous cysts of the abdomen, connected with the ovary, or foreign to that organ: (with regard to cysts of a multiple or areolar description, or complicated with various kinds of degeneration, they are not to be touched).

7th. That iodine injections may also be employed in cysts containing fatty unsanguineous matters, after premising repeated punctures, with the view of transforming such cysts into collections of serous fluid.—*L'union Med.*, Nov. 7, 1856.

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## EDITORIAL AND MISCELLANEOUS.

— The regular meeting of the Academy of Medicine for April was very fully attended, many Fellows being present from a desire to hear the discussion upon Puerperal Fever, appointed for that evening. In

this they were in some measure disappointed, for the stated business of the meeting was set aside for the purpose of listening to an able paper upon the Pleura, read by Dr. Isaacs. This paper was statistical, giving the results of the author's observations upon one hundred subjects, relative to the anatomical relations of the pleura, particularly in reference to its elevation above the clavicle, and its relations to the subclavian artery. The conclusions to which the author arrived, were that the pleura rose higher above the clavicle than is stated by any writer upon anatomy, and that its relation to the subclavian artery is of the greatest importance in all surgical operations of that region.

After the reading of this paper, the Committee appointed at the last meeting to draft appropriate resolutions respecting the death of *Dr. E. K. Kane*, reported. Previous to reading the resolutions, Dr. J. W. Francis, the Chairman of that Committee, requested the privilege of presenting to the Academy some expression of his personal feeling, which it had been his pleasure to prepare. These remarks of the venerable doctor are so full of eloquent and genuine sentiment, that we do not hesitate to insert them in full, as we find them reported to a daily journal.

Dr. Francis proceeded to read as follows:

As a member of the Committee appointed by the Academy to offer for your adoption, should they meet your approving decision, resolutions on the death of the late Dr. Elisha Kent Kane, I can scarcely withhold a remark or two on this melancholy occurrence. The occasion is one of profound consideration. The lamentable fact of his early death has now become widely known, and wherever known is regretted with a sorrow that has scarcely limitation, and is felt as almost a personal bereavement. The expressions of that grief have been already diffused throughout our vast Republic, and never has an individual character, unassociated with the dignities of high official station, received loftier commendation from the national voice. His death seems to have reached the hearts of the entire American people, and will doubtless be sympathized with by the wise and good of the Eastern world. I consider the late Dr. Kane to have been one of the most remarkable men of his time, and to have already been recognized as such. His physical and mental energies are hardly to be found recorded in the ample page of biography. With a constitution singularly delicate and refined, with a corporeal development that gave no special tokens of great muscular strength, of a size below the ordinary height,—slender, susceptible, and of quick movement,—he evinced a power and capacity for laborious service and marvellous endurance in trial beyond that which the stoutest and strongest have shown. Though what might be popularly called a diminutive man, he was of fair proportions, of compact elements,

comely and attractive in appearance. I betray in no wise the confidence of friendship, when I state, what I derived from his own lips just before his last departure from New York, that his greatest weight had been 97 pounds, and that during a portion of his Arctic career it did not exceed 93 pounds.

His intellect was of a peculiar order, alert, rapid in combination, and of large forecast. He was analytic and synthetic. His range of thought was wide, his combinations singularly immediate. The simple and the compound are equally open to his perceptions. Early disciplined in the schools of literary and scientific instruction, with such faculties, his acquisitions were great and various. To a fair scholarship in the ancient languages, he added the sciences of astronomy, navigation, natural history, in its several relations of botany, mineralogy, geology, chemistry, and the departments of physics which fall within the compass of a liberal medical education. He was an artist; he was an ethnologist. He was a searcher after truth, an explorer of the mysteries of nature, and periled life in his devotion, with the daring of a soldier, and the stimulus of a scientific adventurer. He studied philosophy, as the handmaid to useful purposes. His confidence rarely halted, however surrounded with difficulties. On the contrary, unexpected occurrences were no obstacles, but met with new devices, and assurances of hope, presaged in the midst of conflicting issues. He was a mighty traveller; had seen men and cities widely remote; had borne vicissitudes of divers climes, and miraculously survived the assaults of pestilence in its most hidden forms. China, India, and Ceylon, the Pacific and her isles, Egypt and the Nile, loathsome Africa, lovely Greece, Mexico and her antiquities, were his preparatory trials. Nothing that unfolded the handiwork of nature was ever indifferent to him. He contemplated a pebble—he surveyed a pyramid. He was a disciple of God's garden. It was rare indeed that you encountered an individual of his cosmopolitan experience. His conversational talents were most engaging; terse in description and sententious; prompt in utterance and discriminative; he secured an audience at will, while the convictions of his own belief and his ample knowledge led captive the auditor of his eloquent logic. As his speech was glowing, so too we find his writings, clear and vigorous, united with a bewitching rhetoric, at once enlarging our comprehension and fathoming the heart. No writer of fiction has assumed a finer sentiment than he betrays in his narrative of the Arctic explorations. His work has become a fire-side companion, and the icy world grows familiar as the parlor story or the devotional hymn. Youth forsakes his school-boy recreations to study its astounding incidents, and listening old age marvels how long it has been kept ignorant of the wonders of Providence. Tears are shed at the record of his multitudinous hardships—how many have burst from his own anguished eyes at the sufferings of mortals he has met amidst the vicissitudes of his own adventurous life!

Dr. Kane was too guileless to be able to conceal the benevolence of his nature, and his manly sensibility gushes forth spontaneously

whenever the noble instincts of humanity are summoned into action. I am told that he was a rapid writer; that he sequestered himself, after his last return, within the apartments of his library, greatly to the detriment of his already wasted health, in order to bring forth his captivating volumes at brief summons, for the gratification of an anxious public, and it is wonderful to think that while in the Arctic regions, with all its horrors, with his disabled crew, and himself wearied, and the victim of disease, he could have penned such beautiful thoughts, so abounding in imagery and in description, always subjected to a harmoniously religious meditation. How eminently practical he was in every need, in every reverse! How admirably he preserved the integrity of those three special rules which he adopted upon his embarkation! They were rigidly adhered to through all the vicissitudes of the expedition. He adds: "We had no other laws."

I have spoken of his physical powers and of his intellectual qualities: let me glance at those of the heart. That organ which I may say constitutes the microcosm of man in his social state, in Dr. Kane was by nature tender and devotional, and by culture had become possessed of the great elements of Christian philosophy. Modesty and humility found favor within his breast. With a fortitude and a perseverance that would have entitled him to the admiration of a Zeno, he had the winning simplicity and tenderness of the gentler sex. In his most familiar converse no word dropped from his lips at which even fastidious modesty could be annoyed, and to his own transporting achievements he rarely made reference unless to satisfy the curious inquirer; for be it understood that the interests of humanity far outweighed in his generous bosom commercial extension or geographical knowledge. The plaudits of mortals were less in his estimation than the approving smiles of Heaven. He was in the amplest acceptation of the term a philanthropist. While memory retains her seat, so long shall I cherish the recollection of his enthusiastic utterance to his noble friend Henry Grinnell, in reference to the then still hopeful prospect of the recovery of Capt. Franklin and his companions. It yields untold comfort to my anxious mind, involved in the cares of professional toil, sometimes to remember that while a child I had seen Alexander Mackenzie, the first white man who traced his way across the Rocky Mountains and gazed upon the Pacific Ocean; that I cherish a personal knowledge of Capt. Franklin and his companions and biographer, Dr. Richardson; and that Kane, an American explorer, who was the first who came within sight of the great Polar Sea, can be recorded as a personal friend.

The eloquent and able eulogist, Rev. C. W. Shields, in his thoughtful and appropriate address, at the interment of Dr. Kane, has beautifully said: "Let us believe that a faith which supported him through trials worse than death, did not fail him when death itself came." Another intimate friend of Dr. Kane thus expresses himself: "But above all,—governing and guiding all,—there was a spirit of true Christian benevolence and Christian dependence. This it was that nurtured and sustained him in times of his greatest suffering and anx-

iety; this it was that gave him firmness, confidence and power, before which the rudest spirit bowed." Testimony abounds that, though weakened and worn to an extreme degree, his mind rose superior to his physical sufferings, and preserved its faculties entire; that he was conscious of his approaching end, and prepared for the change. His Christian confidence wavered not; the benignity of his nature was edifying throughout all his sufferings; and one of his very last expressions was, that he held in little estimation that man who was afraid to die. Dr. Kane adds another imperishable name to the long catalogue of the members of the noble faculty of physic, who, in their lives and in their deaths, have enriched the panoply of faith by labors in humanity, and who, by their writings, have demonstrated the excellence of the Christian's hope.

The resolutions were then read, and it was voted that they, together with the remarks made by Dr. Francis, should be transmitted to the bereaved family.

This business being completed, the stated subject for discussion then came up, and Dr. Francis, who was appointed to open it, made some brief remarks. He did not think it was necessary to trace the symptoms of puerperal fever, which were familiar to all. He would approach directly the subject of contagion. He avowed himself a decided believer of its contagious character, and its specific origin, and agreed with the older writers, who were all contagionists, that it was a fever like other fevers, differing only in its locality. The difference which now obtains upon this point was commenced by Gordon in 1789, and has continued since. Many modern teachers do not believe in its contagious character, but his experience and observation of forty-eight years duration, has strengthened him in his belief upon this point.

Dr. J. M. Smith then read a long and carefully prepared essay upon the subject, reviewing the opinions of the best authorities upon the causes and mode of propagation of puerperal fever.

The discussion of the subject was interrupted by the reading of a long paper, which we hardly think is doing justice to the spirit and freedom of a discussion. We would much prefer listening to Prof. Smith's own opinions, and to a recital of his own experience, than to a lengthy *rechauffé* of the opinions of a host of writers upon the subject. This, it seems to us, is the intention of a discussion, and when it comes to reading didatic treatises, the benefits and forcible practical results of a discussion are lost.

Dr. Smith can well afford to yield to the spirit of debate which is in him. His language is chaste and elegant, his delivery smooth and forcible, and his reading and experience extensive, while there are

few Academicians who can so readily get and sustain the undivided attention of the Academy. In fact, his extemporaneous opening of the debate upon rheumatism, in March, made that sitting extremely interesting, and was more to his credit than the prepared paper upon puerperal fever.

Upon the conclusion of Dr. Smith's paper, the Academy adjourned to a special meeting to be held in two weeks, for the further consideration of the subject of puerperal fever.

At the special meeting, Dr. Smith again made a few remarks, opening the discussion by recapitulating some few points of his paper, avowing himself a contagionist, and dwelling upon the identity between typhoid fever, erysipelas, hospital gangrene, and puerperal fever.

Dr. Alonzo Clark followed Dr. Smith, agreeing with him as regards the communicability of the disease from one individual to another, but how it was accomplished, whether by the skin or by the respiration, he did not know. He did not believe any relation existed between it and typhoid fever. There had been but six cases of typhus or typhoid fever in Bellevue Hospital for six months; there was but one case now there, and that in the male ward. Yet there was an epidemic of puerperal fever, which certainly could not be attributed to the former disease. Again, the Reports of the City Inspectors, running back for fifty years, show no relation between the two diseases. Nor did he find any analogy in their pathological lesions. His convictions were that further evidence was needed of any identity between typhoid fever and puerperal fever.

It was not so, however, with erysipelas, for he had observed in Bellevue Hospital, that when erysipelas was present in one ward, puerperal fever occurred in another. This opinion was sustained by those who had observed the epidemic erysipelas in Vermont. Statistics also come in to support this belief, for the ratio of mortality is very similar, the fatality of the one increasing in equal degree with the fatality of the other. It was the same with hospital gangrene.

Dr. Clark does not think it is a fever under any circumstances. He has not yet seen a case where there was not some inflammatory lesion present. Dr. Gooch was the first to designate it as a peritoneal inflammation with lesion, while Dr. Simpson thinks it frequently exists without lesion, as does also Dr. Tyler Smith.

Dr. Clark considers that there are four principal lesions. 1. Inflammation of the peritoneum; 2. Inflammation of the veins of the body of the uterus, ligaments, and ovaries; 3. Inflammation of

the lymphatics ; 4. Inflammation of the inner surface of the womb. In all of his cases he had found one or more of these lesions. The first was the most common, accompanied with serous effusion, with effusion of lymph, and ending in purulent effusion. The number where there was inflammation of veins was small. Cruvelhier met seven or eight only during two years and a half. Inflammation of the lymphatics was now frequently found, yet the number of cases was limited.

After parturition the veins upon the inner surface of the womb remain open for a few days. In these veins can be found blood, pus, and when no other lesion is evident, pus may be discovered in the little sacs, found at the union of several of these veins. These cases Dr. Clark calls endo-metritis.

The lesions mentioned are not independent of each other, but are found in connection, as peurperal peritonitis.

Inflammation of other serous membranes are frequently found, as those of the pleura and pericardium.

The same is true with the whole course of the mucous membranes of the intestines. The spleen is rarely changed, as is the case in typhoid fever, while it is common to find serum in the cerebral cavities, and the vessels of the brain, if cut into, will bleed. The blood is coagulated in the heart ; but in many cases where the examination has been made almost immediately after death, it was not cogulated in the vena cava. The ovaries and graafian vessels are sometimes found infiltrated with pus and serum. The primary lesions are in the organs of generation ; the secondary lesions the result of blood poisoning.

Dr. Clark declined entering, at the time, upon the discussion of the treatment, and was followed by Dr. Reese, who objected to the direction the discussion had taken. With the exception of Dr. Clark, he said, the disputants had confined their remarks to the subject of contagion. He thought it was necessary to define the meaning of puerperal fever, as it had been used to include various inflammations of peritoneum, metritis, hepatitis, &c. As for the contagiousness of puerperal fever, he did not believe in it, nor had he as yet seen a single case in which the local lesion found would not account for the disease.

The discussion was continued by Drs. J. M. Smith, Clark, T. F. Cock, and Corson, rather in the form of interrogatives directed to Dr. Clark, who briefly replied. The sitting of the Academy was continued till a late hour, and finally adjourned without touching

upon the question of treatment, which was deferred to another meeting.

We have thus given, briefly, the principal points which were brought up in the course of this discussion.

With the exception of Dr. Clark, all the Academicians confined their remarks to the subject of contagion and the relation existing between puerperal fever and typhoid fever, erysipelas, and hospital gangrene. They were very nearly of a unanimous opinion upon the question of contagion—Dr. Reese alone dissenting. Dr. Smith considered the nature of the disease to be that of fever. Dr. Clark, on the contrary, did not admit of its febrile character, but considered it as an inflammation.

The treatment of puerperal fever, which will probably be discussed at the next sitting of the Academy, will doubtless afford some interesting statements. We trust that the whole subject of the opium treatment, which has been so highly commended, may meet its elucidation from every Fellow, who has had the sad necessity of resorting to it.

—It is proposed to hold a convention of Opthalmologists in Brussels this year. We have received, from the editor of the *Annales d'Oculistique*, the following Circular, setting forth the plans of this convention, which we are requested to publish.

*Convention of Opthalmologists.*

Honored Sir:

The committee of publication of the *Annales d'Oculistique* have resolved to convoke, at Brussels, a *Congrès d'Opthalmologie*, to which all those physicians, of every country, who cultivate this branch of the medical sciences, or who are interested in it, are invited. The convention will meet the 13th, 14th, 15th, and 16th of September next, that is to say, immediately before the opening of the Congress of German Physicians and Naturalists, which is to be held at Bonn, from the 18th to the 25th of the same month.

Impressed with the advantages to result from this reunion, and with the conviction that they will be understood and appreciated by you, the committee of organization of the convention hope for your presence, and for your valuable concurrence.

Without intending to arrange at this time any programme, it is highly probable that the question of military Opthalmia, a disease which for so many years has desolated many armies of the continent, and which is increasing each day in populations in which it unfortunately is propagated, will be an important topic in its deliberations. Exact statistics of this serious disease, in different countries, the manner of its introduction, the examination of the measures proper to check its progress, of the results obtained or to be expected from their application, the discussions of the curative indications, and the means to meet them, can not fail to throw some light upon its

history, now imperfectly known, and to exercise a salutary influence upon its prophylaxis and its treatment.

The Opthalmoscope, that ingenious means of diagnosis hitherto not generally used, ought to be more thoroughly appreciated in its applications. Each of the members of the convention, bringing to the solution of the questions which are allied to it, the fruit of his studies and his experience, and the aid of clinical demonstrations, will give to its propagation a desirable impulsion, at the same time fixing with precision the limits of its use.

Finally, special sittings will be given to the exposition of particular facts, exclusive of those mentioned in the programme, upon which the members may judge proper to call the attention of the convention.

We think it useless to enter into details, which we very well know would now be premature. Aided by the counsel of competent men, we hope to be able to draw up, very soon, a programme in harmony with the designs of the congress and the requirements of science.

We trust that you will be willing to second our efforts, by informing us, *as soon as possible*, both of your acceptance of this invitation, and of the subjects to which you desire the attention of the convention to be called.

*Committee of Organization.*

FALLOT, President of the Royal Academy of Medicine of Belgium, &c., President;

BOSCH, Surgeon to the Opthalmic Institute of Brabant, &c.,
HAIRION, Director of the Opthalmic Institute of the Army, at Louvain, &c.,
VON ROOSBROECK, Director of the Opthalmic Institute of Brabant,
} Members.

WARLOMONT, Editor of the *Annales Oculistique*, Secretary.

*Brussels*, January 15, 1857.

N. B.—Physicians who do not receive this Circular expressly directed to them, will please consider themselves invited by this notice, and will have the kindness to send their replies to the committee.

Communications will be received in all languages.

The Editors of the AMERICAN MEDICAL MONTHLY will be happy to forward communications which any of their readers may be pleased to make to this convention.

—The *Quarterly*, which so much resembles the ship-of-the-line *Pennsylvania* of its native waters, has employed a man in Connecticut, who writes books for children, to make a savage attack on Dr. Green. That gentleman will, we hope, be as much benefited by this as he has been by previous assaults from the same source; but our advice to "W. H." is, *ne sutor supra crepidam*.